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EDITORIAL NOTES

"INHUMAN COMEDY."

Any one who thinks that the editorial note in the last issue entitled "New State Insane Asylum" was harsh and unkind to our noble and intelligent (?) legislators (and we have been informed that there are a few such critics), ought to read the editorial in the San Diego *Union* of May 25, 1913. "Yorick," the writer in the San Diego *Union,* thinks the legislators were fools; is it not more charitable to regard them as merely insane? Here is some small portion of what he writes; unfortunately we have not space for all of it:

> "I have been trying to review the antics of the legislative circus from the mental attitude of a spectator in the reserved section of a top seat midway between the two rings. (It was a two-ringed affair, populated mostly by clowns.) But the circus folk, including the trained monkeys and the educated pigs, were so absurdly foolish that a fellow-feeling compelled me to throw my own cap into both rings and mix with the freakish aggregation; they appealed so irresistibly to my sense of the ridiculous that I couldn't resist taking part with them in their tomfoolery. There has been nothing like it on the face of the earth since Nebuchadnezzar ate grass in Babylon; there has been no folly quite so viciously imbecile since King David prayed the Lord to turn the counsel of Ahthopol into foolishness to the end that it might not corrupt Absalom conspiring against his father's throne; the only parallel I can find in Scripture is in that remarkable passage declaring that the princes of Zoan are become fools, the princes of Noph are deceived by the wise men of Pharoah who have seduced Egypt, even they that are the stay of the tribes thereof. I am something of a connoisseur in foolishness, and I am always intensely interested when I am fortunate enough to find an especially perfect specimen of the folly bird. It was beyond my fondest expectation, therefore, when I met up with that Sacramento bunch of Merry Andrews and consorted with the contents of the legislative menagerie."

Is that bad?

LAWYERS' AND DOCTORS' FEES.

If you consult a lawyer about a transfer of some lot, or the signing of a deed or a contract or something of the sort involving, let us say, only a thousand dollars or so, he will charge you from $25 to $any-old-number. But the lawyer will take his pregnant wife to a doctor for an opinion—which he gets for from $2.50 to $10; or his child, whose life or death may depend upon the diagnosis and advice given by the physician, and if the doctor charges more than ten or twenty dollars the lawyer will be furious. We forget that it is not our moments of time that we are selling to our patients—or more often giving to them—it is our brains, our thought, our experience, the years of suffering we have felt—and seen—and which we have so heartbreakingly tried to prevent. Perchance it is just because we have suffered so much in ourselves and our patients and feel first for the life of the patient, that it leaves us the poor fools that we are, unable to put a charge upon our advice that would in a measure pay us for the hours of work, anxiety and study that have made it possible to give that advice. Who can put a price on life; on health; on happiness? Least of all the man who is called upon to save and conserve these priceless things, life and health and happiness; for what is life worth without health—and can there be happiness without it? How often do we see the millionaire enjoying his money and his estate and the physician, who saved to him that life, struggling to pay a mortgage on his humble home. Let us, without becoming unduly commercial, try to learn to put a relative value upon everyday things.

TELEPHONE EXCHANGE; AN ERROR CORRECTED.

Some members got the erroneous idea from an editorial in the last issue that the telephone exchange in San Francisco, which is working well and is very useful, was a place where anyone could have a doctor recommended. That was not the idea at all. We took up the suggestion made in the *Journal A. M. A.*, that such a physicians' telephone exchange would be of the greatest use *in an emergency.* For example, a serious accident occurs at Fillmore and Geary streets and a number of people are injured; doctors are needed immediately. Who knows what physicians live in that section, or are at home, or can be reached quickly? No one. But the exchange has a list of physicians who can be reached at once; out of several hundred doctors

on the list it is easy to go down the line, ringing up one after another and getting those who are at liberty and will go to the scene of the accident. That was the idea expressed and it is difficult to see how any person could get any different meaning out of it, yet such seems to be the case. As a matter of fact, the exchange never recommends a physician; it exists merely to locate the particular physician that some patient may want to reach. Any undertaking that operated differently would not survive, for no one wants to pay to belong to an exchange that plays favorites.

DIVISION OF FEES; THE LOS ANGELES TREATMENT.

The Los Angeles County Medical Association has adopted a plan to fight this petty graft of fee-splitting which may possibly do some good. They print a list of their members and place an * against the name of each member who has stated that he did not split fees. Some of those who have so stated and whose names are so marked may lie, but at any rate they are then known to be liars by at least one other person, and they may have some shame. Do you think it would be a good idea to try this in the case of the membership of the State Society and to print in the Register and Directory, which is issued every year, an * against the names of the members who agree in writing not to split fees? It might do some good; it could not possibly do any harm; would you like to have it tried in the next edition? If so, write to the Secretary what you think about it and he will gladly place your views before the Council at its next meeting. Anything that offers even some small relief from this pickpocket form of dishonesty would seem to be worth trying.

DISHONEST BUSINESS BY MEDICAL (?) JOURNALS.

In San Francisco and in Los Angeles there has recently been much scandal connected with the police departments; policemen, it appeared, had taken a portion of the money which bunco men got from innocent victims. It is a contemptible business, this petty larceny graft, yet it is also in our own profession in those two cities; owners of medical (?) journals are taking their "whack" of the bunco money the promoters of worthless nostrums get from their innocent victims, largely aided by the medical (?) journals which advertise these things. This has been going on for years. Over and over again the STATE JOURNAL has been asked why it did not criticize the *Pacific Medical Journal* and the *Southern California Practitioner*. Ten years ago, when the STATE JOURNAL was in its first year and the fight against dishonest drug preparations had just been started by us, the question came before the publication committee and it was then agreed that the STATE JOURNAL should not criticize any journal published in California until we had become firmly established and no charge of "envy" or "jealousy" could be raised with the least shadow of probability behind it. It has not been easy at all times to live up to that rule, but still it has been done. It was very hard to live up to it when we saw a typical "write up" of the old style, giving a report of some cases of tuberculosis "cured" by the use of that disgusting fake, "dioradin," appear in the *Southern California Practitioner*. It has not been easy to keep silent when letters have been received asking why the names of some of our members were printed as "associate editors" of these publications that were and are carrying disgraceful nostrum advertising, and the STATE JOURNAL had no word of criticism. It has not been easy not to ask whether any one is really fool enough to pay money to subscribe for a publication like the *Pacific Medical Journal*, each issue of which is principally made up of mushy reprinted stuff and bunco advertisements, in which respect it is a little worse than the *Southern California Practitioner*. If either one of these publications really has 100 actual paid subscribers, then would it seem true that "doctors are the easiest suckers that are!"

The *Southern California Practitioner* was so loaded with nostrum "ads" that, several years ago, some of the men whose names had been carried as "associate editors" requested that the use of their names be discontinued; and it was done. Something over a year ago a new editor took charge of the *Practitioner* and when the subject of the character of the advertising was discussed with him, he said that he was going to drop the notoriously bad advertisements as soon as the contracts ran out; a moth-eaten excuse. To what extent he has complied with his expressed intentions, anyone who can find a copy of the publication can see for himself. The advertising solicitor of the *Southern California Practitioner* was in the office of the STATE JOURNAL only a few weeks ago and made the ingenuous remark that they would take anything that paid and that he did not know of any advertising contract having been rejected! Just money; anything for money! How the physicians whose names are given as "associate editors" of the *Southern California Practitioner* and the *Pacific Medical Journal* can stand that notoriety, it is difficult to see. Perhaps they do not know that their names are being so used, and for that reason we will not print them in this issue.

"THE COLLEGE."

"The name of the corporation is the College of Surgeons," so says the official announcement on page 4, I; but in III it says "The corporation is to be known as the College," and thereafter it refers to itself as THE College. There are titles enough, God knows, to suit the taste of the most fastidious hungerer after titles and letters. We learn that there is to be a Board of Governors consisting of the first five hundred to be invited to attend the emporium by the original Murphy-Martin committee; they are to be known also as Founders of the College; all other ordinary mortals are to be merely Fellows of the College; thus you see the first bunch get two nice titles right at the jump off. There is also to be a Board of Regents selected from the Board of Governors; these lucky mortals will thus have an additional title; and it seems unfair that only twelve should be so blessed

by the Murphy-Martin providence. But there is some small crumb of encouragement for us little fellows; for $25 and $5 a year, we may perchance become a "Fellow" and if such is our luck, just think what we can do: "All Fellows of the College shall be designated a Fellow of the College of Surgeons and shall be authorized and encouraged to use the letters F. C. S. after his name on professional cards, in professional directories and in scientific articles published in surgical literature." It does not say whether the big one of the elect is to be encouraged to use these letters after his name in the articles which may be published in newspapers, thus informing the public of the wonderful discoveries and how he can make the crippled-for-life walk and run about. There is to be still further segregation: "The prospective Fellows are to be divided into four classes, A. B. C and D." The natural interpretation of these cabalistic letters would be the last thing that THE College, or the Regents, or the Governors or any of the muchly bedecorated officials would ever think of; we fear they have no sense of humor. Class "A" one would suppose would indicate Fellows especially handy with the Appendix; class "B" should point out to the incontinent or the suppressed a Fellow who is keen on the Bladder; class "C" might be used to designate those of the Fellows who are highly Commercial and notorious fee-splitters; of course, it is obvious that the man with an ingrowing toe nail will have to pick a Fellow from class "D"—or one who does Divers odd jobs. John Jones can now, if he is lucky enough to be liked by someone who was liked by someone who was liked by the Murphy-Martin "committee," have a brand new lot of stationery printed as follows: "John Jones, M. D., F. C. S., F. of C. S., R. of C. S., G. of C. S., class A (or whatever it may be)." Is there a patient who could get untangled from that string of letters and go to some other less distinguished surgeon? We rather guess not! If that accident should occur, the Regents of THE College will undoubtedly fix up some more titles so as to get some more letters. But think of the state of mind of the poor man with a bellyache who thinks it is appendicitis and sends a messenger boy out to get the card of a real Fellow, goes painfully down the list of letters till he comes to the end and then finds that he has got a "B" Fellow or a "D" Fellow instead of an "A" fellow! Shocking! Oh you Fellow!

REMEMBER TO LOOK THROUGH THE ADVERTISING PAGES OF YOUR JOURNAL.

REMEMBER TO SEND US YOUR CHANGE OF ADDRESS PROMPTLY.

ORIGINAL ARTICLES

INTRATRACHEAL INSUFFLATION ANESTHESIA.*

By SAXTON TEMPLE POPE, M. D., San Francisco.

Experimental physiology often paves the way for the advance of surgery. In the field of thoracic operations, it not only paved the way, it forced surgery to follow. Physiologists had demonstrated the possibility of maintaining artificial respiration and pulmonary ventilation many years ago. Vesalius, in the sixteenth century, first used the laryngeal tube to produce inflation of the lungs. In animal experimentation, Legallois, Monroe, Magendie and Marcy all resorted to an apparatus for artificial respiration, using a tracheal cannula. Surgeons followed rather tardily, employing measures such as the Sauerbruck cabinet, the Tiegel positive pressure apparatus, Green's apparatus and similar devices. The list of experimenters who have attacked the problem is a long one: Matas, Fell, Hans Mayer, P. J. Murphy, Vidal, Karewski, Brauer, Janeway and Robinson, Engelken, Willy Meyer, Elsberg, Boothby, Eisenberg, Peck, Pool, Cotton and many others, all contributing something to the general knowledge of the subject. Sterling Bunnell invented a very ingenious positive pressure mask.

But it remained for Meltzer and Auer of the Rockefeller Institute to originate and popularize the successful method now under consideration. Their work met all the fundamental requirements of the situation. They established the facts that pulmonary ventilation might be maintained by a constant stream of air or oxygen, under definite pressure, being blown in the trachea. This insured the proper oxygenation of the blood, inflation of the lungs, favored the continuance of cardiovascular circulation and permitted, if desired, the induction of narcosis by means of a volatile anesthetic.

All of this is done with a comparatively simple apparatus, easy of operation and absolutely sure in its action. At one move it abolishes the cumbersome, uncertain appliances of the past and opens the thorax to the progress of surgery.

The work of Meltzer and Auer, Elsberg, Flint, Janeway and others has proved that intratracheal anesthesia is not only a successful solution of an important phase of intrathoracic surgery, but is a safe adventure. They even claim that it is safer than the usual surgical narcosis.

Intratracheal intubation eliminates the danger zone—that region lying between the lips and the pulmonary alveoli—where so many of the problems of obstructed respiration have their origin. At the same time it establishes and carries on continuous artificial respiration. The patient cannot die from respiratory failure. This immediately abolishes a large percentage of all anesthesia mortalities.

That most delicate and readily disturbed of all essential functions, respiration, that which quickest shows impending shock, and most elusively departs in the crisis of acapnia, is under the positive control of the anesthetist.

Elsberg has proved that we need fear no damage

* Read before the California Academy of Medicine, January 27, 1913.

from the intratracheal tube. Pneumonia, tracheitis and bronchitis are not more frequent after its employment than after any other anesthesia.

Supported by these facts, and having in view the further progress of surgery in this direction, we have, at the University Hospital, constructed a machine which embodies the essential features of a fully developed intratracheal insufflation apparatus.

At this work, Dr. Mary Botsford and I have collaborated under the direction of Dr. Terry.

Utilizing the chassis or framework of a little Tiegel apparat, which had been imported by the late Dr. Bush, but which already had become obsolete through the rapid progress of anesthetics, we laid the foundation for our machine. On this movable base we set an ordinary electric motor and pump, such as is commonly used to fill air tanks for dentists or nasal specialists. From this pump we conduct the air through metal-wound tubing to a large wash bottle, containing and surrounded by warm water. This serves not only as a storage tank for the air, giving a more steady stream, but warms, washes and moistens the atmosphere which

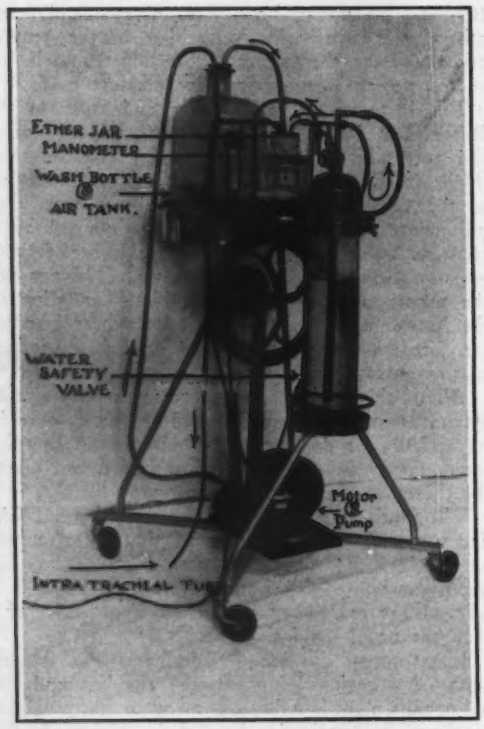

FIG. I.

must later be forced into the lungs. From this large bottle the air is conducted to a simple arrangement of nickeled tubing and stop cocks, which is so constructed that the air may pass either straight ahead, or go through the ether jar, or do both. As it goes through the ether jar it but skims the surface of the ether. There is no tube leading down under its surface. Carrel, in his laboratory experiments, found that this is all sufficient when the ether and air are warmed. And it absolutely prevents the ether by any chance being sucked up into the tubing and injected into the lungs. This accident has happened at least once in the human in the East, and several times in our experience during the experimental stage of the machine we have seen it occur in animals.

Before the air comes to the ether jar there is a side track which leads to the safety escape. In this machine this is a water valve. We use it here simply because it was already on the Tiegel apparat. This escape prevents the pressure rising above a certain mark, and permits the regulation of pressure simply by raising or depressing the movable tube which is submerged in the water.

As the filtered, warmed, moistened etherized air is about to enter the final course of tubing on its pulmonary mission, it passes one more restriction—a mercury manometer. Here the pressure is gauged. Physiological experiments have shown that it requires an air pressure of 15 to 20 mm. mercury in the machine fully to inflate the lungs; more than this is dangerous in the opened thorax, and even less is necessary in work on animals. Here a greater pressure, even in the closed thorax, produces rupture of the alveoli and a retro-peritoneal emphysema. This was first demonstrated at the U. C. Research Laboratory by Tait. The ether jar is surrounded by a second jar in which hot water is kept if desirable. Sometimes, when the patient requires a large amount of anesthetic, this added warmth is necessary to increase the volatilization of the ether.

We can with our machine raise the temperature of the etherized air in the delivery tube from 25° C.—the room temperature—to 48° C.—a temperature too great for safety. Experience has taught us that water at 60° C. in the jars gives the required 36° in the catheter.

There is nothing original in our machine. We have simply taken the things at hand and applied them to the requirements of the case. The essential is a constant flow of warm, moist air, carrying varying proportions of ether, under a pressure adjustable from 10 to 20 mm. mercury, and incapable of rising above this.

We have also constructed another machine—much less cumbersome, having the pump separate, a rubber bag to equalize the pressure of air, smaller wash bottles, mercury safety valve, electric lights to produce warmth, and a monovalve regulator for the air and ether.

Both machines also have a stop cock for the attachment of an oxygen tube, so that this may be run through the ether or serve if the pump should break, and to be used at the last of the anesthesia to blow out the ether, thus quickly rousing the patient from his narcosis.

Having your machine and your patient, the next thing is to connect them. This is done in the adult by the introduction of a No. 21 or 22 F catheter into the trachea, a distance of 25 or 30 centimeters from the teeth. This is no easy matter—in our experience. It is advisable to have administered a preliminary injection of morphine and scopalamine, and then to have the patient thoroughly anesthetized with ether.

In this condition, after inserting a mouth gag, the first or second finger is put well down the throat, the epiglottis located and pushed well forward; then the introducer, loaded with the disconnected catheter, is passed along the finger into the rima glottidis. As soon as the catheter is in the larynx, you are made aware of the fact by the hissing of air in the tube and the coughing expiration of the patient. A silent, quiet insertion is characteristic of a miss: an esophageal catheterization. When the tube really is in the larynx, it is pushed down well and the introducer slipped off; then the connection is made with the machine. It is well to have the machine running all the time, so that no interruptions occur in the anesthesia and respiration. Sufficient air escapes around the catheter in the trachea for all purposes of pulmonary ventilation.

We have tried the introduction of this tube with the assistance of the laryngoscope, but have found it more difficult with the patient on the operating table than we anticipated.

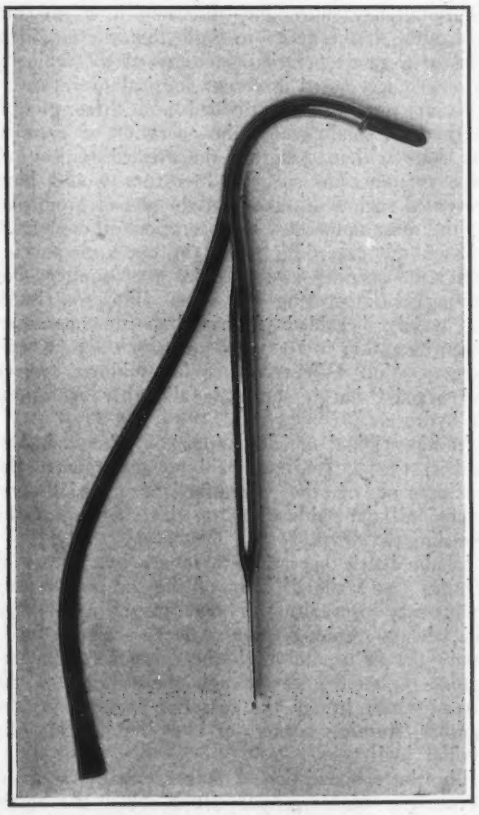

FIG. II.

In some subjects, where the jaws are projecting and there is more or less ankylosis, the introduction by touch is very difficult; but as we gain experience, it becomes progressively a more simple process. The introducer is simply a large hollow sound, permitting the passage of a 23 F catheter. Boothley I believe invented it.

Our catheters are not boiled, because this softens them, but they are cleaned in a phenol solution and lubricated with vaseline.

Some of our early patients complained of sore throat after recovery from the anesthesia, and a little laryngitis; but this we believe was due more to the unsuccessful attempts at intubation than to the irritation of the tube when in place.

Anesthesia proceeds smoothly from this point on. The patient can quickly be brought under control of the anesthetic. Respiration continues along with the insufflation in the lighter degrees of narcosis. In more profound sleep, respiratory excursions are abolished, without particular danger to the patient. They return naturally upon lessening the narcosis or after stopping the air current. This extreme phase, however, is seldom necessary and need not be approached unless desired. At any time the ether may be cut out entirely and only air or oxygen administered.

At the termination of the operation the patient can be roused within two or three minutes by blowing him out with oxygen.

The tracheal tube is left in until breathing is good, and a slight coughing warns us of the return of the laryngeal reflex.

We have used this apparatus in eighteen cases at the University Hospital. Only two of this number have been thorax cases. But we have found it of unquestionable service in operations upon the tongue and jaw. Heretofore, even under the smoothest anesthesia, these operations have been punctuated by coughing, choking, obstructed respiration, reflex laryngeal spasm, inspiration of blood and interruptions without number. With intratracheal anesthesia they become a quite easy performance. The blood takes care of itself. There is no danger of inspiration of clots or respiratory failure.

Meltzer and Auer have computed that the proper size catheter for insufflation is one that fills one-half the aperture of the larynx. If increased obstruction be necessary to inflate the lungs fully at the closure of a thoracic operation, compression of the throat above the thyroid cartilage will serve the purpose.

We have not tried the Killian laryngoscope to assist the intubation. Possibly this would be a marked advantage.

The Sewell mouth gag, when tried, was no help in adults. With children the epiglottis can be seen plainly with this instrument in place, which facilitates the intubation very materially.

In the experimental laboratory we use it constantly as a method of anesthesia, and there it certainly reduces the anesthesia risk. It is practically impossible to kill an animal under intratracheal anesthesia, unless it be bled to death. Cardiac death is exceptional in laboratory animals. Respiratory failure was an ever present menace.

Carrel has used the same form of anesthesia for several years, and it has made possible some of his most astonishing experiments. A number of these we have duplicated in our laboratory, and we expect a definite advancement in surgical research from the employment of this very useful, safe, and indispensable method of anesthesia.

RECURRENCE OF HYDROCELE AFTER RADICAL TREATMENT.

By DUDLEY TAIT, San Francisco.
From the laboratory of surgical research of the University of California.

Ten years ago the radical treatment of hydrocele was *res judicata.* Eversion and total resection shared the favors of surgeons.* To resection were assigned the relatively rare cases of symptomatic vaginalitis and chronic pachyvaginalitis. All other varieties were treated by eversion. Of late, however, the advantages claimed for eversion have been questioned, numerous recurrences being reported after its use. Hence a return to the so-called method of total resection of the tunica vaginalis, which our German friends persist in calling Bergmann's operation, although it was practiced by Celsus, Albucassis, Ambroise Paré, Dupuytren and others.

An extensive personal experience with eversion and a careful study of many published cases of recurrence following this operation have convinced me that the great majority of said failures were due to (1st) errors in technic; (2nd) errors in the choice of the operative procedure. The further elucidation of these statements is my excuse for the present causerie.

In the operation of eversion as originally performed by Vautrin, Doyen and subsequently Winkelman, no attempt was made to approximate the edges of the everted serosa. The frequency of recurrence soon reported from all sides led Legueu and Jaboulay to stitch the edges of the everted tunica as high as possible around the cord by means of two or three catgut sutures. Even then the redundant serosa would occasionally get free, sagging in the direction of the testis, the folds of the serosa adhering to each other, producing small pockets, thus inviting recurrence.

Longuet (1900) perfected and simplified the operation of eversion by doing away entirely with all dissection, omitting the delivery of the tumor and making a new bed for the testicle.

Under local anesthesia, a transverse fold of the scrotum over the testicle is taken up and cut with the scissors down to the serosa which is immediately taken up and cut in like manner. Neither incision should exceed three or four cm. By means of pressure from behind the testicle is made to escape from its serous sac. It is then lifted upward and forward between two fingers, avoiding traction on the cord which will invariably give rise to pain and nausea.

The different scrotal layers (tunica and overlying fibrocellular tissue) will be seen to retract in the direction of the cord, the posterior surface of which comes directly into view. Note that there has been no dissection, no delivery of the tumor.

The edges of the everted tunica are stitched together by means of two or three catgut sutures passing through the cellular tissue of the posterior surface of the cord. The testicle is not replaced in its original position, but a new bed is provided for it in the middle of the inner edge of the incision, by inserting both index fingers into the loose connective tissue adjoining the raphé and then rapidly separating them a distance of five or six cm. This step is generally painless; it is always bloodless. The testicle is then carefully dropped into the resulting cavity, which is inside of and parallel to the old cavity. In its new position the testicle is slightly twisted on its axis, being in retro-lateral version instead of in normal antero-version. The scrotal wound is closed by means of Michel staples or with a Cushing stitch.

It is uncommon to keep the patient in bed after the first day; indeed, I have seen patients walk home two hours after this operation and resume work the next day. With a properly fitted suspensory the resulting local reaction is habitually insignificant. Mobility of the testicle may be noticed as early as the third day. It is almost invariably present at the time of removal of the staples. Close examination will then show an anterior attachment to the testicle.

My sole excuse for referring in detail to a procedure already thoroughly discussed in a previous publication is the desire to show that the modifications of a good operation have caused the majority of recurrences noted in recent surgical literature.

Recurrences may be divided into three groups. In the first small group, the operation of eversion was done without securing the everted tunica. A mere reference to surgical literature would have prevented such a mistake. In the second group, by far the most numerous, the operation differed from that of the preceding group in one unimportant point: the opening in the tunica was smaller; but here again no suturing was done. This constitutes the widely heralded Andrews' bottle operation, which its author in 1907 and again in 1912 (Keen's Surgery, Vol. IV) recommended without reserve and urged "that it supersede all other operations for hydrocele."

In his original article (*Annals of Surgery,* December, 1907), Andrews paid no attention to the development of the operation of eversion and ignored all the publications on this subject for the preceding six years.

In the bottle operation there are two errors of technic; (1st) the delivery of the sac is a totally unnecessary procedure; it may cause hemorrhage and always prolongs the patient's rest in bed; (2nd) the failure to stitch the everted tunica predisposes to recurrence through the sagging of the serosa, which brings the secreting serous surfaces together, forming pockets or even reproducing the original hydrocele.

The latter condition I have actually seen in operating a recurring hydrocele in which the bottle operation had been done two months previously. I had noticed a similar state of affairs in one of my cases in 1899 when eversion was practiced without stitching the everted serosa.

Recurrences after the bottle operation were reported by Eastern surgeons several years ago (Lyle, Moschowitz, etc.), and of late the list of failures seems to be increasing.

In the third group of recurrences after eversion

* I firmly believed that the injection method had been relegated to history; recently, however, I learned that it still constitutes the treatment of choice at one of our local teaching hospitals.

are the cases of hydrocele due to subacute infections, tuberculosis of the epididymis, chronic pachyvaginalitis. Eversion failed me in two cases belonging to this group (one unknown infection and one tuberculosis), but it succeeded admirably in several cases of pachyvaginalitis.

Experience, however, has proved the superiority of excision in the majority of hydroceles belonging to this group. Recurrence after the so-called total excision of the tunica was reported by Boyer in the latter part of the 18th century. This is easily understood when one remembers the anatomy of the region: the testicle lies outside of the closed serous sac; consequently, in the so-called total excision, that part of the tunica covering the testicle is left untouched, and may, under certain pathological conditions, continue to excrete, just as a cyst in any part of the body may reproduce itself after accidental tearing has occurred during its removal.

It was the latter consideration that led Bartlett, the clever St. Louis surgeon, to advocate the excision of the unopened hydrocele thus disposing of every particle of the excreting surface. Bartlett described his procedure in 1909, as follows: "After turning out the sac in the ordinary manner it is easiest to begin its removal at the spermatic cord; the loose tissue connecting these two structures can readily be separated by blunt dissection, as can the tumor from the testicle everywhere except at its lateral reflections from that organ, where some cutting must be done. No fluid need be lost and one will be surprised at the ease and quickness with which the dissection can be accomplished. Some small vessels will have to be ligated."

I have never performed this operation, and I know of no one who has resorted to it in other than thin wall hydroceles, that is to say, in the class of cases where it is unnecessary—a cure generally following a much simpler and safer procedure: eversion. Conservatives have accused the radical treatment of hydrocele of having caused a long list of disorders (epididymitis, testicular sclerosis, atrophy, sterility, dystrophy). Clinical and post-mortem experience have apparently ruled out all of these claims.

According to Charrin, Moussu, LePlay and Corpéchat, Ancel and Villemin, the role of the serosa is not merely mechanical; it has a trophic influence on the subjacent organ. From their experiments on sheep and guinea pigs they conclude that eversion or excision of the tunica invariably leads to marked interstitial and peripheral testicular hyperplasia, fatty degeneration and atrophy of the seminiferous tubes. As proof, however, that infection played a very important part in these experiments the following may be cited:

1. The presence of testicular adhesions even as late as a year after the experiment.

2. The predominance of the areas of atrophy on the surface of the testicle.

Hermann, after some very faulty experiments, concludes that the reaction following the radical treatment of hydrocele habitually causes the loss of the testicle, "although regeneration may take place."

From my own animal experiments I am convinced that the protective role of the tunica vaginalis, like that of other serous membranes, has been greatly overestimated. Among the results of these experiments (eversion, resection, injection of irritants), the following are of interest:

1. Infection of the tunica is invariably followed by a marked change in the testicle; reduction in size, sclerosis and peripheral areas of atrophy.

2. Under strictly aseptic conditions eversion is not followed by atrophy of the testicle; the adhesions that occur between the testicle and the surrounding cellular tissue generally disappear within a few weeks when the testicle becomes free and normally movable as if within a new cavity. A peritesticular sclerosis is invariably present; there is a thickening of the albuginea, but the sclerosis does not extend into the parenchyma or involve the epididymis or the vas.

3. The testicular sclerosis is not more marked after eversion than following the injection of irritants into the tunica; it is frequently less pronounced.

4. Excision of the tunica gives rise to more marked testicular reaction than does eversion.

5. That the function of neither the interstitial nor the spermatogenic cells is affected by bi-lateral eversion of the tunica vaginalis is sufficiently proved by the total absence of developmental abnormalities in and the multiplication of puppies after said operation.

6. It would seem, nevertheless, that the testicle is no exception to the law of general pathology relating to the creation of points of lessened resistance by traumatism or infection.

Conclusions.

1. Although eversion is only a palliative measure and does not reach the determining factor, it will, when properly performed, prove eminently satisfactory in over 90% of hydroceles. The medium size, thin wall, chronic hydroceles are the most favorable for eversion.

2. Longuet's method of eversion, *without delivery of the sac,* is the simplest, safest, and least liable to recurrence. It frequently succeeds even in very thick wall hydroceles. A few symptomatic hydroceles recur after eversion.

3. Recurrence frequently results from failure to stitch the edges of the everted tunica vaginalis.

4. Andrews' bottle operation is a failure; it is a step backward in the history of the therapeutics of hydrocele, and its adoption accounts for a large proportion of the recurrences noted in this country.

5. Excision is preferable to eversion in the rare cases of chronic pachyvaginalitis. The so-called total excision of the tunica vaginalis is not total, and recurrences following its use have been reported.

6. Excision of the unopened hydrocele is the only complete method of removing the entire excreting surface. It has not been resorted to in thick walled hydroceles; in thin wall hydroceles it is an unnecessarily complicated procedure.

7. Of the numerous objections made by conservatives to the radical treatment of hydrocele

none resists either a thorough clinical or the experimental test.

8. The protective role of the tunica vaginalis has been overestimated.

9. Under strictly aseptic conditions experimental eversion of the tunica is not followed by atrophy of the testicle; it may produce a mild peritesticular sclerosis.

Discussion.

Dr. R. L. Rigdon: The paper of Dr. Tait does not leave much to be said in the way of discussion for it has covered the ground so thoroughly that but little can be added to it. The best that I can do is to simply mention some personal experiences that I have had with the operations for hydrocele, and none of these experiences are out of the usual. As a rule hydroceles are so easily taken care of that we do not consider them at all seriously; they are easily disposed of in one or other of the ways known to us. Years ago, when Keyes proposed as a remedy the injection of carbolic acid, he stated that it was a painless method and that the patient could go away from the office after the injection without being confined to bed. I was much impressed by his reports and began to use the injection method. At first I was afraid to use carbolic acid in quantities sufficient to produce the desired results, but having gained confidence, I used more acid, was more careful in the technic and the results were satisfactory. So far as I know, there has been but one untoward result, and that occurred years ago in an old man. In this instance sloughing of the scrotum occurred. I always felt that it was my fault rather than that of the operation, because it was employed in an unsuitable case. In properly selected patients the results will be satisfactory. At the clinic, if we desire to employ the injection method, we draw off the fluid, inject the acid and send the patient home. We have him return the next or the following day and draw off any inflammatory effusion that may have occurred. I now recall no failures to cure, but I realize that failures may occur.

With reference to the matter of eversion of the testicle, one has to do that operation but once or twice to realize that something more than opening the sac is necessary; the work must be seasoned with a little surgical judgment. If the sac is not opened high enough but a closed space is left at the upper portion, this may readily refill. A specimen procured for me by Dr. Howard Somers illustrates a condition that is sometimes present and which if overlooked might give rise to recurrence. I refer to the little pockets that may be found between the epididymis and the testicle. Occasionally these are quite large and the opening into them is small and after the eversion operation these pockets might close and form cysts in which reaccumulation of fluid would take place. Ordinary judgment would indicate that if the sac were large it is good surgery to cut off a large part of it. The hemorrhage that occurs is very easily controlled. In connection with trimming the sac off, unless one is careful one might cut uncomfortably close to the vas deferens but this mishap need not occur if one will bear it in mind. I have had no personal experience in transplanting the testicle but we know that it can be done without injurious results. As to the effects of the various operations on the function of the testicle, I think they are nil.

Dr. M. Krotoszyner: Unfortunately I was too late to listen to Dr. Tait's paper, but I recall a previous paper of his, in which he said that the eversion method for hydrocele can be easily done so that the patient can get up within two days after operation. I cannot verify this by my results; it takes, in my own work, about a week before the patient is up and about. As regards the operative treatment of hydrocele I think it is wisest to offer the patient, at the present stage of our knowledge, the radical operation, which consists in either excision of the tunica—after Volkmann, or the eversion of the tunica around the cord after Winkelmann. In those cases where the tunica is much thickened and diseased I have held to the first procedure as the better method. In those instances, though, where the tunica was only moderately diseased, I have resorted to a combination of both methods of everting the tunica after removing part of it and stitching the cut-edges around the cord. I have followed a good many of my cases as regards end-results and must say that I have not seen so far any relapse of hydrocele after the careful performance of any of these radical operations. For uncomplicated cases of hydrocele we can look for no better, quicker or safer operation than the eversion method.

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THE NATURE OF DISEASE.

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There is difficulty in giving a clear idea of what is meant when we speak of disease although every individual has a concept of the word when applied personally. In a way, the difficulty might be met by simply regarding disease as the negation or opposite of the normal, but the same difficulty applies to the definition of normal. The trouble is in giving a definition which will apply to the species when all our concepts of the conditions are formed from individual experience. It is possible to conceive a normal type of a species by taking the average of weights and measurements at definite ages and the average or usual reactions which the living material exhibits under the influence of external conditions. The structural or anatomical normal type is easier to construct than the physiological or functional, although no two individuals have ever been found in whom all of the compara-

tively coarse measurements made under the Bertillon system agree. How much the physiological type varies is evident when we think of the individuality of our friends. Variability is one of the most striking of the attributes of living things and in which they differ from lifeless bodies. There is nothing individual in a block of marble or in a mass of table salt. The incredible complexity of living material in both chemical and physical structure would render it well nigh impossible for two individuals of a species, even one of the simplest, to be in all respects the same. All that we know justifies us in the assumption that the reactions of a material of the same character and under the same conditions will always be similar, and the variations in reaction by which we recognize individuality indicate differences in structure. We are accustomed in forming both the structural or anatomical and the functional or physiological type of a species to set certain arbitrary limits within which the measurements must fall.

An individual may be said to be healthy when there is such an harmonious adjustment to the environment that the reactions exhibited will be the usual for him and will correspond with the reactions of the species. In disease the reactions differ from the usual, the body temperature may not be the same, indefinite discomfort or even sensations of pain may be experienced, or the mental reactions may differ. Just as individuality is due to underlying structural differences, so disease is due always to certain underlying structural alterations which have been produced in various ways, and in consequence of these structural alterations the reactions exhibited are no longer those usual for the individual or species. It is possible then to define disease as a change produced in living things in consequence of which they are no longer in harmony with the environment. It is evident that such a conception of disease is inseparable from the idea of life; only a living thing can become diseased. In a dead body there was a preceding disease and in consequence of the change or injury produced by disease, that particular form of activity which constitutes life has ceased. Changes such as putrefaction take place in the dead body, but these are changes which would take place in any mass of material similarly constituted and are not influenced by the fact that the mass was once living.

As in the consideration of disease we are dealing with living things, it may be well to consider for a moment some of the most important attributes of living matter. I have already spoken of its complexity of chemical and physical structure in consequence of which variability becomes more easy. Not less striking is its other great attribute, adaptability, in consequence of which the reactions can become so changed that it becomes adapted to an environment which is not the usual. Living matter occurs always in the form of small masses called cells which are the living units. They vary in form, structure and size, some being so large that they can be distinctly seen with the naked eye, while others are so small that they cannot be distinctly seen with the most powerful microscopes. Each cell contains within it a specially differentiated material called chromatin which is usually aggregated in a mass called nucleus. This chromatin exercises a regulatory function, over the processes of cell life, and to its action the transmission of heredity is due. The living thing or organism may be composed of a single cell, or in the case of the higher animals and plants of great numbers of cells those of the same character being combined in masses to form organs such as the liver or the brain. It is generally believed that the cellular material has a gelatinous or semi-fluid consistency and within the cell is contained within a network. In each cell there are a number of chemical substances of great complexity of molecular structure, and the whole is an extraordinarily complicated structure from every point of view. In a simple cell of not more than 1/2000 of an inch in diameter there is a more complex organization and more different sorts of activities in progress than would be embraced in the most complex modern household. Certain conditions are essential for the continued existence of living material. It must be surrounded by a fluid or semi-fluid medium in order that there may be interchange with the environment. Having in itself no source of energy, it must constantly receive from the outside a supply of energy in the form of food, and waste substances, formed as the result of the intracellular chemical activity, must be removed. In the case of the more complex animals formed of cell masses, it is evident that there must be certain arrangements by which food may be conveyed and the activities of the several organs co-ordinated to the purposes of the entire organism, and this is effected by the circulation and the nervous system. In the case of many animals, it would seem as though the necessity for a fluid environment did not apply, for the superficial cells of the skin have no fluid around them; these cells are, however, dead and serve a mechanical or protective purpose only.

A simple conception of health and disease can be arrived at by the study of these conditions in a unicellular animal directly under a microscope. For this purpose a small organism called amoeba, which is commonly present in fresh water ponds, may be used. This appears as a small mass seemingly of gelatinous consistency with a clear outline, the exterior part homogeneous, the interior granular. The nucleus which is seen with difficulty appears as a small vesicle in the interior. Many amoeba show also in the interior a small, clear space, the contractile vesicle, which alternately contracts and expands and which probably assists in the movements of the intra-cellular fluid and in excretion. The interior granules often change their position, showing that there is motion within the mass. The amoeba slowly moves along the surface of the glass by the extension of blunt processes formed from the clear outer portion which adheres to the surface and into which the interior granular mass flows. This movement does not take place by chance, but in definite directions and may be influenced. The amoeba will move towards certain substances which may be placed in the fluid and

away from others. In the water there are usually other organisms, particularly bacteria, on which the amoebae feed. When a bacterium comes in contact with an amoeba, it is taken into its body by becoming enclosed in processes which the amoeba protrudes. The enclosed organism lies in a small clear space in the amoeba surrounded by fluid which has been shown to differ in its chemical reaction from the general fluid of the interior. This clear space which may form at any point in the body corresponds to a stomach in a higher animal and the fluid within it to the digestive fluid or gastric juice. After a time the enclosed organism disappears, it has undergone solution and is assimilated; that is, the substances of which its body was composed have been broken up, the molecules rearranged and a part has been converted into the substance of the amoeba. If minute insoluble substances such as particles of carmine are placed in the water, these may also be taken up by the amoeba; but they undergo no change and after a time they are cast out. Under the miscroscope only the gross vital phenomena, motion of the mass, motion within the mass, the reception and disintegration of food particles and the discharge of inert substances can be observed. The varied and active chemical changes which are taking place cannot be observed.

Up to the present the environment of the amoeba is that to which it has become adapted and which is favorable to its existence. Under these conditions its structure conforms to the type of the species as do also the phenomena which it exhibits, and it can assimilate food, grow and multiply. If, during the observation, a small crystal of salt be placed in the fluid, changes almost instantly take place. Motion ceases, the amoebae appear to shrink into a smaller compass, and they become more granular and opaque. If they remain a sufficiently long time in this fluid, they do not regain their usual condition when placed again in fresh water. None of the phenomena which characterized the living amoebae appearing, we say they are dead. After a time they begin to disintegrate and the bacteria contained in the water and on which the amoebae fed, now invade their tissue and assist in the disintegration. By varying the duration of the exposure to the salt water or the amount of salt added, a point can be reached where some but not all of the amoebae are destroyed. Whether few or many survive depends upon the degree of injury produced. Much the same phenomena can be produced by gradually heating the water. It is even possible to gradually accustom such small organisms to an environment which would destroy them if suddenly subjected to it, but in the process of adaptation many individuals will have perished.

It is evident from such an experiment that when a living organism is subject to an environment to which it has not become adapted and which is unfavorable, such alterations in its structure may be produced that it is incapable of living even when it is again returned to the conditions which are natural to it. Such alterations of structure or injury are called the lesions of disease. We have seen that in certain individuals the injury was sufficient to inhibit for a time only the usual manifestations of life; these returned when the organism was removed from the unfavorable conditions, and with this or preceding it the organisms, if visibly altered, regained the usual form and structure. We may regard this as disease and recovery. In the disease there is both the injury or lesion and the derangement of vital activity dependent upon this. The cause of the disease acted on the organism from without, it was external to it. Whether the injurious external conditions act as in this case by a change in the surrounded osmotic pressure, or by the destruction of ferments within the cell or by the introduction into the cell of substances which form stable chemical union with certain of its constituents and thus prevents chemical processes taking place which are necessary for its life, the result is the same.

The experiments with the amoebae show also two of the most striking characteristics of living matter. 1. It is adaptable. Under the influence of unusual conditions alterations in structure and possibly in substance may take place in consequence of which the organism, under such external conditions, may still exhibit the usual phenomena. The organism cannot adapt itself to such changes without undergoing change in structure although there may be no evidence of such change visible. This alteration of structure does not constitute a disease provided the harmonious relation of the organism with the environment be not impaired. An individual without a liver should not be regarded as diseased provided there can be such an internal adjustment that all of the vital phenomena go on in the usual manner without the aid of this useful and frequently maligned organ. 2. It is individual. In the varying degrees of exposure to unfavorable conditions some, but not all, of the organisms were destroyed; in the slight exposure few, in the longer many. Unfavorable conditions which will destroy all individuals of a species exposed to them must be extremely rare.* There is no such individuality in non-living things. In a mass of sugar grains each grain shows just the same characteristics and reacts in exactly the same way as all the other grains of the mass. Individuality, however expressed, is due to structural variation. It is almost impossible to conceive in the enormous complexity of living things that any two individuals, whether they be single cells or whether they be formed of cell masses, can be exactly the same. In no way is the individuality of living matter more strongly expressed than in the resistance to disease. The variation in the degree of resistance to an unfavorable environment is seen in every tale of shipwreck and exposure. In the most extensive epidemics certain individuals are spared, but here care must be exercised in interpreting the immunity, for there must be differences in the degree of exposure to the cause of the epidemic. It would not do to interpret the immunity to bullets in battle as due to any individual peculiarity, save

* They do, however, take place, since within comparatively few years whole species have completely disappeared; for example, the great auk and the passenger pigeon. In these cases it is not known what part disease played in the destruction.

possibly to a tendency in certain individuals to remove his mass from their vicinity, and in battle and in epidemics the factor of chance enters. No other living organism is so resistant to changes in environment as is man, and to this resistance he owes his supremacy. By means of his intelligence, he can change the environment. He is able to resist the action of cold by means of houses, fire and clothing; without such power of intelligent creation of the immediate environment the climatic area in which man could live would be very narrow. Just as disease can be acquired by an unfavorable environment, man can so adjust his environment to an injury that harmony will result in spite of the injury. The environment which is necessary to compensate for an injury may become very narrow. For an individual with a badly working heart, more and more restriction of the free life is necessary until finally the only environment in which life is even tolerably harmonious is between blankets and within the walls of a room.

The various conditions which may act on an organism producing the changes which are necessary for disease are manifold. Lack of resistance to injury, incapacity for adaptation whether it be due to a congenital defect or to an acquired condition, is not in itself a disease, but the disease is produced by the action on such an individual of external conditions which may be nothing more than those to which the individuals of the species are constantly subject and which produce no harm.

When the causes of disease are considered in their entirety, it is found that they are extrinsic and act on the body from without, and it is important to understand the relations which the body of a highly developed organism such as man has with the world external to him. These relations are effected by means of the various surfaces of the body. On the outside is the skin, a surface of large extent and many times increased by the various glands connected with it, for a gland, however complicated in structure, is nothing more than an infolding of a surface. This external surface communicates at two places, the mouth and the anus with the great internal surface formed by the lungs and the intestine. The internal surface of the lungs is estimated at 98 square yards, this large extent being due to the extensive infoldings just as a large surface of thin cloth can, by folding, be compressed into a small space. The intestinal canal from the mouth to the anus is thirty feet long, the circumference varies greatly, but an average circumference of three inches may safely be assumed which would give a surface of between seven and sight square yards, this being many times multiplied by adding the surface infoldings and the internal surfaces of the large glands such as the liver and pancreas which are connected with it. There is a third surface, the genito-urinary surface, formed by the urinary and genital canals and which communicates with the external surface by one opening only.

The simplest of the diseases to understand are those known as the infectious diseases which are caused by living organisms which enter into the body and living at the expense of it cause disease. The entry into the body is effected by means of the surfaces mentioned.

Such diseases play an enormous part in the life of man, the majority of deaths are caused directly or indirectly by infection, no other diseases have been so much studied and in no other department of science has the knowledge obtained been capable of such direct application in promoting the health, the efficiency and the happiness of man. This knowledge has added years to the average length of life, it has rendered possible such great engineering works as the Panama Canal, and has contributed to the food supply by making habitation possible in large and productive regions of the earth, which formerly, owing to the presence of conditions causing disease, were uninhabitable. It would not be too much to say that our modern civilization is dependent upon this knowledge. The massing of the people in large cities, the factory life, the much greater social life which are all prominent features of modern civilization would be impossible were there no means to control the infectious diseases. Means of communication, and the rapidity of communication, and the general movement of people which have developed in equal ratio with the massing, would serve to widely extend every local outbreak of infection.

These diseases appearing in the form of great epidemics have often been a leading factor in determining historical events, for they have led to the defeat of armies, the fall of cities and of nations. War is properly regarded as one of the greatest evils that can afflict a nation, since it destroys men in the bloom of youth, at the age of greatest service, and brings sorrow and care and poverty to many families. But the most potent factor in the losses of war is not the death in battle, but the deaths from disease. If we designate the lives lost in battle, the killed and the wounded who die, as 1, the loss of the German army from disease in 1870-71 was 1.5, that of the Russians in 1877-78 was 2.7, that of the French in Mexico was 2.8, that of the French in the Crimea, 3.7, that of the English in Egypt, 4.2. The total loss of the German army in 1870-71 from wounds and disease was 43,182 officers and men, and this seems a small number compared with the deaths in the same period in Prussia alone, 129,128, from smallpox. In the Spanish-American war there were 20,178 cases of typhoid fever with 1580 deaths. In the South African war there were in the British troops 31,118 cases of typhoid with 5,877 deaths, and 5,149 deaths from other diseases, while the loss in battle was 7,582. The Athenian plague which prevailed during the Peloponnesian war, 430-425 B. C., not only caused the death of Pericles but according to Thucydides, a loss of 4800 Athenian soldiers and brought about the downfall of the Athenian hegemony in Greece. In the Crimean war between 1853-56, 1600 English, 80,000 French, 800,000 Russians died of typhus fever. The plague contributed as much as did the arms of the Turks to the downfall of Constantinople and the Eastern Empire in 1450. It was the plague which in 1348 overthrew Sienna from her proud position as one of the first of the Italian cities and the rival

of Florence, broke the city forever, leaving it but a phantom of its former glory and prosperity. The work on the great cathedral which had progressed for ten years was suspended and when resumed it was upon a scale adjusted to the diminished wealth of the city, and the plan restricted to the present dimensions. It is rarely that any thing is all to the bad, for the same plague saw the birth of the novel in the tales of Boccacio, which were related to a delighted audience of the women of the Renaissance who had fled from the plague in Florence to a rural retreat. The knowledge which has come from the study of infectious disease has served also to broaden our conception of disease, it has linked more closely to medicine such sciences as zoology and botany, it has given birth to the sciences of bacteriology and protozoology and in a way has linked all sciences more closely together. Above all it has made medicine scientific, and never has knowledge obtained been more quickening and stimulating to its pursuit.

There are various kinds of living organisms which are capable of producing disease and the most important of these considering the number of diseases which they produce, and the great role which these diseases play in both man and animals, are the bacteria. These are simple cells of very elementary structure and having great power of growth. They vary greatly in size, shape and capacity for growth. The smallest of those producing disease is the influenza bacillus which is 1/50,000 of an inch in length and 1/100,000 of an inch in diameter, and among the largest is a bacillus producing an animal disease, which is 1/2000 of an inch long and 1/2500 of an inch in diameter. The rate of multiplication varies with the species and with the environment and under the best conditions it may be very rapid. A generation—that is the interval between division of a single organism into two—has been seen to take place in twenty minutes. At this rate of growth from a single cholera bacillus 16,000,000,000,000,000 would be formed in a day. Such growth, however, is extremely improbable under either natural or artificial conditions both from lack of food for the organisms and from the accumulation in the fluid in which they are growing of waste products which check growth.

From the simplicity of structure, it is not improbable that the bacteria are among the oldest forms of life and all life has become adapted to their presence. They are of universal distribution, they play such an important part in the inter-relations of living things that it is probable life could not continue without them; at least, not in the present way. They form important food for other unicellular organisms which are important links in the chain of living things; they are the agents of decomposition by which complex substances are reduced to elementary substances and made available for use; without them plant life would be impossible for it is by their instrumentality that material in the soil is so changed as to be available as plant food; by their action many of the important foods of man, often those especially delectable, are produced; they are constantly with us, on all the surfaces of the body; masses live on the intestinal surface, and the excrement is largely composed of bacteria. It has been said that life would be impossible without bacteria for the accumulation of the carcasses of all animals which have died would so encumber the earth as to prevent its use, but the folly of such speculation is shown by the fact that animals would not have been there without bacteria. It has been shown, however, that the presence of bacteria in the intestine of the higher animals is not essential for life. The coldest parts of the ocean are free from those forms which live in the intestines and fish and birds inhabiting these regions have been found free from bacteria; it has also been found possible to remove small animals from the mother by Caesarian section and to rear them for a few weeks on sterilized food, showing that digestion and nutrition may go without bacteria.

Considered in its biological relations infection is adaptation of an organism to the environment which the animal body offers. In certain cases the environmental adaptation is very narrow; for example, when an organism is parasitic for a certain animal species only, in others the adaptation may extend to a large number of genera. The bacteria may be adapted to an exclusively parasitic existence, or they may find conditions favorable for existence outside of the body as well. Infection can also be considered as a part of the struggle for existence between living things, organims seeking to enter into and live upon the tissues of the body. The most favorable condition for the organism is when very little injury to the host is produced by its growth; the death of the host is an unfortunate incident in that the particular family branch of the parasite which is living harmoniously in the host may be cut off.

All of the surfaces of the body offer resistance to the invasion of the bacteria. There are natural areas of less efficient protection on the surfaces, on the skin, for instance, where the hairs emerge and the protection may be further impaired by slight injuries of the surface.

The chief danger from wounds is that their surfaces may become infected. Death from wounds is due more frequently to infection than to the actual injury represented by the wound. Much depends upon the character of the wound. Infection of clean wounds made by a sharp cutting instrument from which there is abundant hemorrhage with sealing of the edges of the wound by clotted blood rarely happens. Typical wounds of this sort are extremely common in shaving and infection of such wounds is extraordinarily rare. If with the wound pathogenic organisms are placed in the tissues, or foreign substances such as bits of clothing are carried in with a bullet, for example, or if the instrument causing the wound be of such a character as to produce extensive lacerations of tissue, infection is more apt to occur. The less frequency of infection in modern wars is in part due to the simpler character of the wounds and in part to the fact that modern fixed ammunition is practically free from germs. The old spear head, the arrow, the square-headed bolt, had little regard for the probabilities

of infection. Whether infection follows a wound depends both upon the entry of pathogenic organisms and upon these finding in the tissues suitable opportunities for growth. In the case of wounds in which there is much laceration of tissue organisms find the most favorable conditions for development. The very slight wounds produced by the exploded cap in the toy pistol give suitable conditions for the development of the bacillus, producing tetanus or lockjaw. The deaths of children from lockjaw following a Fourth of July celebration have often exceeded the total deaths in a South American revolution. The tetanus bacillus is a widely distributed organism, whose normal habitat is in the soil and which is usually present on the dirty hands of little boys. The toy pistol wounds are made by small bits of paper or metal being driven into the skin. The wound is of little moment, the surface becomes closed, and a bit of foreign substance, a few dead cells and the tetanus bacillus from the surface remain enclosed and in a few days the fatal disease develops.

The body has means of defense even when the invasion by bacteria has taken place. One method of defense is by what is termed phagocytosis, certain cells of the body having or acquiring the power of devouring bacteria. The bacteria may also be destroyed by becoming dissolved in the fluids of the body. The struggle between the body and the parasite may be likened to a combat in which each combatant has means both of offense and defense. The bacteria act by the production of poisonous substances which destroy or inhibit the activity of the cells and tissues, and the body produces substances which are antagonistic to the bacterial products. What are termed the symptoms of an infectious disease, the fever and the evidences of inflammation as shown in swelling and redness, are but the indications of the struggle which is taking place. The infectious diseases are preeminently those of the first half of life. The diseases of organs such as the heart, the kidneys and the liver are chiefly in the last half of life. These organic diseases have a close relation to the infections in that in the course of disease an injury to an organ may be produced which becomes operative not at once but later. We must not think that the parasites are the exclusive external causes of disease; any environmental condition to which the body cannot adapt itself and which in consequence produces injury is a cause of disease. It would seem at first sight that diseases produced by tumors, that malformations and that old age give examples of disease due not to external but to internal causes. Old age cannot in itself be regarded as a disease, for the alterations which we find in the body are those usual for that period of life of the species. In the case of malformations due to imperfections in embryonic development, the embryo developing within the uterus of the mother is not removed from the action of conditions external to it, and tumors cannot be considered as yet in relation to cause, for we know nothing concerning their cause.

The influence of the knowledge of disease has been far reaching. In every system of religion which man has found time and inclination to formulate the power to combat disease, to heal the sick, has been a test of its efficiency. In the far past disease has generally been thought of as coming to us from beyond the domain of which we have knowledge, by sense impressions, and as due to conditions outside of the knowledge and control of man. The veil of mystery which enveloped it has been torn away; most diseases are recognized as due to conditions which are definitely within the control of man although there are inherent difficulties in the exercise of that control. Nor is this knowledge confined to the medical profession. The main facts of disease are becoming common knowledge and the interest of the people is aroused as never before; the daily newspapers devote a considerable part of their space to information concerning disease, which information is too often false and conveyed in a sensational manner. Many of the longer articles in the magazines on various diseases are admirable and are important means for the education of the people, and the modern novelist has also become conscious that there must be some control over the imagination in depicting diseases. The heroine no longer faints so frequently as she did in the early Victorian period, nor is a sudden reverse of fortune so apt to produce brain fever in the hero. The general recognition that disease like gravity and chemical attraction follows the operation of definite laws is gradually leading to a weakening of the belief in supernatural directing agencies.

It is not fully recognized how dependent our modern civilization is upon the control of disease. The essential principle in this is the substitution of mass action for individual action, resulting in the most minute division of labor. This has necessitated a vast increase in factory life and the factories being usually located in cities a swarming of people into the cities, the attraction being the more exciting and interesting life and fancied superior means of livelihood. Individual effort as opposed to the effort of mass with minute subdivision of labor has become progressively less efficient. The infectious diseases in the past would have made impossible the modern extension of the factory and the increased development of city life. Think what would happen in a factory the employees of which were unprotected against smallpox in which a single case of the disease were introduced.

Another great development has been the increase of communication among people. In the days preceding steam, isolation was the safeguard of the people. Wars were usually associated with and followed by epidemics, diseases being generalized in camps and disseminated along the paths of armies and by returning soldiers. Pilgrimages and any movement of the people contributed to disease. Epidemics had a fatality unknown now which was probably in part due to the fact that the isolated communities acquired no resistance to diseases which were foreign to them. In the days of sailing vessels voyages were so long that an epidemic of disease acquired at a port would exhaust itself on the voyage before the vessel reached another port. In the rapid steam communication diseases tend to lose endemic characteristics and to become gen-

eralized. There is a tendency not only to loss of individuality in nations as expressed by dress, customs, tradition and belief, but they lose their characteristic diseases and receive others in return. Only those endemic diseases which are transmitted by insects which have a strictly local habitat remain endemic although the region in which these diseases are endemic may become greatly extended. The most striking example of this is in the case of sleeping sickness which starting in a focus in Nigeria has extended along the routes of the rubber traffic throughout the entire Congo region and to East and South Africa, wherever the transmitting flies are found. The distribution of insect carriers of disease may also take place and is greatly to be feared; possibly it has taken place but time would be necessary for slow breeding insects to establish themselves. But in all cases with the knowledge of a danger some means of opposing it will be found. Not only has there been such a general dissemination of the diseases of man and animals, but the increase of communication has led to dissemination of the diseases of plants as well. Formerly, the plant species were restricted in their extent. The farmer for the main part saved and sowed his own seed and the garden plants did not travel far; there was a limited plant exchange in the immediate neighborhood and even the florist supplied a narrow area only. Now we levy on the world for our plants and seeds. Any plant desirable for beauty of foliage and flower which has developed in any part of the world and there established harmonious relations with the insect and plant life and adapted itself to the immediate parasites is distributed everywhere. In this way there have been introduced plant parasites, the most conspicuous examples being the brown tail and gypsy moths whose ravages in the Eastern states have produced damage extending into the millions and with which we have not yet learned to cope successfully. These two parasites are extending slowly, but surely, but with the extension there seems to be a slow adaptation of plant life to the new environment which they bring. It is not a wild thought that some human parasite may be introduced which in the new environment may find conditions so suitable for its existence that we will be as little able to cope with it as we have been with other new parasites. There have been many examples of the almost incredible power of multiplication of an animal or plant in a new environment where it is removed from the operation of conditions which held it in check, as for example, the introduction of the mongoose into Jamaica, the rabbit into Australia, the Scotch thistle into South Wales and the sinple water plant chara into England.

There has also been a change in the medical point of view, one effect of the change being the gradual elimination of medical sects. It is difficult to see how there can be a theory of disease with treatment based on that theory with any more reason than a theory of chemistry or of physics. There have been certain reactions to the modern scientific conception of disease. The movement has been so rapid that it has produced as rapid rivers will do,—back eddies along the banks. Such reactionary movements are inevitable; individuals who mentally are scarcely at ease in a current moving slowly are stimulated to reaction when the current becomes more rapid and make efforts against it. Such movements may be productive of good; they force us to consider carefully our attitude; some become stronger in their faith, some are carried into the back eddies. The most marked of these reactionary movements is represented by Christian Science. This combines many elements of strength; it has a very definite faith, a book in which with slight grammatical changes from time to time the principles and rules of the faith are recorded, up to a short time ago a living prophetess whose large estate has finally emerged from the legal struggle between her son and the church trustees. It is a curious commentary of this remarkable age that it is a profitable enterprise to found a successful religion. The theory of Christian Science is a total negation of the principles of science for it admits of no doubts nor further investigation. Its organization is admirable, it has the advantage of the control of large funds and a publicity bureau which conducts an excellent daily paper which has a large circulation within and outside of the faith. It is difficult at the present time to say what influence this movement has exerted upon medicine; influence it must have had, for the faith based on certain theories of disease which ally it with sin is embraced by great numbers of people, otherwise intelligent. It is too soon for an accurate determination of its effect but some good may have come out of it. It has much strengthened the knowledge that diseases (with exceptions) are self-limited and tend to recovery; it has shown that many ills are not real, but imaginary and due to a faulty interpretation of impressions; that real ills are often capable of relief by the substitution of other impressions, or by modifying in various ways the conceptions of ills; finally, that pain may be influenced in a great many ways. Many persons are undoubtedly healthier and happier than they would be without the faith. It is also difficult to say how much harm it has done. Many lives and, sad to say, many of these were children, have been needlessly lost from infectious diseases and other conditions which scientific medicine would have relieved. The process of medical education of the laity has been somewhat checked and the finances of many physicians have been diminished. As an offset to the financial loss, the physicians should be grateful to a faith which has relieved them from attendance upon a number of incredibly tiresome people who sought relief from ills which were largely imaginary and the result of idleness. A new and in many respects a nobler conception of medicine has arisen. Formerly, medicine was almost exclusively a personal service rendered and paid for, and measures looking to the public relief and to the prevention of disease received scanty consideration. The few health officers that there were in the chief cities drew salaries and published statistics of health conditions based on mental impressions of population, of births and deaths. Now the dominant idea in the medical profession is that of a wider service to the city, to the state, to the nation, to humanity, rather than

the service to the single individual. This is seen in the establishment of laboratories by boards of health in city and state in which the knowledge obtained by exact investigations can be made available in the service of the people; in the medical inspection of schools and factories; in the passing of laws directed against conditions which affect the public health; in the increased extension of hospitals. It must be remembered in connection with this that as a nation we are but following other nations, never leading, and our public health measures are far behind those of most of the European countries. Think how much is done by the medical profession as represented by the American Medical Association. As a profession medicine has never been one of the most lucrative, standing far behind the law in average income of its members; yet, it gives to the committee which has charge of public health, including public instructions, which we regard as an essential feature of this, $29,000 yearly, and there can be no selfish or ulterior motives behind the gift for the diminution in disease which we believe it furthers is not to the financial benefit of the members. It would be as though lawyers should devote a similar sum of money to the simplifying of the law and the creation of measures by which justice might be furthered. The idea of public service also underlies the creation of special laboratories and institutes in which through research greater knowledge of disease may be obtained and made available. Let us sum up the record of recent achievement; it is a proud one. Human life has been lengthened, many more individuals reaching the age of middle life. This has been due to the control of the infectious diseases which are the diseases of early life. With this has come an increase in the earning capacity of the race. For certain of infectious diseases there has been discovered a definite cure by the use of which the period of disease is shortened and the mortality reduced; for others, means have been found of increasing the resistance of the individual and infection is prevented, others are resisted by diminishing the opportunities for infection, this by the recognition and isolation of cases in the early stages of disease. The researches which have been made on the nutrition of man and the nutritive values of foods are of great importance and have not yet begun to be applied as they should be. It will undoubtedly be possible to realize a great economy in this regard. The greatest demonstration of what it is possible to achieve in the way of prevention of disease has been given in the building of the Panama Canal. In what had formerly with right been regarded as the most unhealthy region of the earth, great numbers of people from different regions, all unacclimated, that is, with no hereditary or acquired adaptation to the local conditions, have been assembled, have been engaged in the most arduous work and the mortality returns show as low a death rate as in the most favorable countries. The record which Col. Gorgas has established by the application of the laws of prevention of disease must remain one of the proudest achievements of man.

THE MEDICAL TREATMENT OF GALL-BLADDER DISEASE.*

By DUDLEY FULTON, M. D., Los Angeles.

Diseases of the biliary tract are somewhat peculiar, in that the same lesion in one individual may be latent and harmless, and in another intensely active and full of danger. With such variegated clinical manifestations and prognostic possibilities, one is shorn of the inclination to offer dogmatic rules of treatment. Individualization is necessary and therapeutic decision can be made only after careful consideration of all factors bearing upon each case.

One of the first things to consider in discussing the treatment of gall-bladder disease is what conditions are medical and in what sort of cases is surgery indicated. While opinions still vary on some points as to the indications for medical or for surgical treatment, it is rather unusual, we believe, for the internist and the surgeon to disagree when considering any given case. It is conceded that certain phases of gall-bladder disease are distinctly surgical, such as acute suppurative cholecystitis, frequently recurring gall-stone colic, empyema of the gall bladder, common duct obstruction, carcinoma, and the pancreatic lesions that are secondary to biliary infections. In certain mild cases—and these are the most frequent clinical types—chronic cholecystitis without stones, pericholecystitis and gall stones that are pursuing a latent course, it is debatable if medical treatment does not offer as much to the patient as surgery. Nowhere do we find wider dispute as to surgical indications than among surgeons, some advocating that there is no other treatment than operative; others, notably Kehr, consider eighty per cent. of gall-bladder diseases as medical.

We have referred a goodly number of patients with mild chronic cholecystitis to the surgeon the past few years, with rather disappointing results; although brilliant results were often obtained, failures have been frequent enough to teach us increasing conservatism in the selection of surgical cases. Not infrequently the post-operative history has been but little happier than in patients who declined operation and have since been carried along by medical treatment.

In diseases of the upper abdomen that have both a medical and surgical side, particularly chronic duodenal ulcer and chronic gall-bladder disease, the surgeon is wont to attribute non-cures to the complications of delay. While this is unquestionably true, the same may be said with double force by the internist. Were all cases diagnosed early, it is probable that medical treatment of gall-bladder disease would arrest the inflammation and prevent sequelae.

The greatest deficiency that exists in clinical medicine and surgery to-day is in diagnosis. Because of this deficiency, many of the cases of gall-bladder disease which now come to us present the end results of infection which began years

* Read before the Forty-third Annual Meeting of the Medical Society, State of California, Oakland, April, 1913.

ago when these conditions were not recognized as frequently as to-day.

There is always a tendency for end results of any treatment to be measured by percentage of mortality, rather than by percentage of cure. The removal of gall stones does not cure gall-bladder disease, unless conditions favoring stasis and infection,—the factors that made gall stones possible —are at the same time overcome. Other failures are due to neglect of post-operative medical treatment. It is ordinarily the custom, after the surgical treatment of nephrolithiasis, to institute treatment to correct those disturbances of metabolism that produce this condition by dietetic measures, free water drinking, and controlling the reaction of the urine, to prevent its recurrence. It seems to us that frequently the same care is not given to the post-operative management of gall-bladder disease.

Diseases of the gall-bladder are of remarkable frequency,—probably only a small percentage of those affected ever have active symptoms. In the Mayo clinic, eight per cent. of women operated for other conditions had gall stones. Many patients after a period of activity subside into latency indefinitely. This would make it appear that latent gall-bladder disease is not a very serious affair and that in such instances a legitimate choice may be offered between medical and surgical treatment. Nothnagle, evidently inclined to this view, once remarked that gall stones belong neither to the physician nor to the surgeon, but to the patient. But, in admitting the foregoing, no internist should forget the potential danger in gall-bladder disease and that needless delay in advising surgery converts such treatment from safety and simplicity to one of complications and inexcusable mortality. There is no better rule to guide the physician than to refer to the surgeon all cases in which the symptoms are persistent or are frequently recurring in spite of medical treatment, for surgery is undoubtedly the most effective treatment of the end results of bile duct infections. Unfortunately, many cases are not cured even by surgical treatment because of the impossibility of controlling adhesions and other gross pathological lesions. Like in chronic appendicitis, the damage is already done and can not be fully rectified.

It would therefore seem that there is a distinct medical side to the therapy of gall-bladder disease and that Kehr was probably not far from the truth when he stated that surgical treatment is indicated in only two types of cases. First, those with "vital" indications, chronic obstruction of the common bile duct, acute and chronic empyema, perforation, and carcinoma. Second, those with "relative" indications, all those cases in which long continued symptoms or frequent attacks of colic have robbed the patient of enjoyment of life, or have endangered his ability to earn a living.

In discussing medical treatment it should be made clear at the outset that the object should be, not the dissolution of stones or the removal of adhesions or the expulsion of concretions from the bile passages, for such attempts are futile. Indications point rather to the prevention and control of infection and stasis—the factors which make these gross lesions possible. These represent the end results of gall-bladder disease and when once formed all that can be expected of medical treatment is palliative. In the ability to reduce gall-bladder activity to latency, medical treatment accomplishes much, since this, as has already been pointed out, amounts practically to a cure in many cases.

Coming now to the prophylaxis and to the treatment of early gall-bladder disease, we find that these phases of the subject are purely medical. In the prevention of the involvement of the biliary tract in acute infections, no extended work from bacteriological standpoint has, so fas as we know, been done. Some clinicians have directed attention to typhoid and colon bacillus infections of the gall-bladder, hoping thereby to prevent or cure the incipient infection of the mucosa before this becomes extensive enough to obstruct the cystic duct, formation of calculi, or the involvement of tissues beyond the mucosa. Engelbach, in his study of this phase of the question, emphasizes the frequency of cholecystitis during the course of typhoid fever and believes that this local infection of the gall-bladder is probably the most frequent cause that prolongs the disease more than three weeks and that otherwise modifies the clinical course of the disease. He cites a series of fourteen cases in which the usual signs of rigidity and tenderness in the gall-bladder region were present, and in which gall-bladder antiseptics and vaccines were used in cases which failed to terminate by lysis the third week. No attempt was made to draw any further lesson from his limited number of cases than a suggestion to stimulate similar work.

Biliary Antiseptics—Crowe's studies determined that hexamethylenamin, when administered in sufficiently large doses (at least seventy-five grains a day) appeared in the gall-bladder in a concentration sufficient to render bile an unsuitable media for the growth of bacteria. In some of the cases, in which the infecting organism was B. typhosus, it was possible to render the interior of the gall-bladder sterile so long as active therapy was continued. Quite recently Burnham has failed to corroborate Crowe's findings and doubts the value of the drug in biliary infections.

Of the numerous drugs employed as biliary antiseptics, we know of none that can be given in sufficient dosage to reach the gall-bladder in a concentration sufficient to be clinically effective. The medical treatment of infections of the biliary passages must be attempted by other measures. Whether or not immunizing serum will prove competent in keeping the bile passages sterile in acute general infections remains for the future to determine.

The extended use of proprietary gall-bladder remedies by the profession is a matter of regret and it is unfortunate that the medical treatment of gall-bladder disease is so frequently limited to the use of such nostrums. Members of the pro-

fession who prescribe such—the composition and pharmaceutical action of which they know nothing, except the gilded promises of the manufacturer —exhibit quite the same gullibility as do those weak members of the laity who, with more faith than reason, become converts to various forms of charlatanism.

Chologen, a representative of this class of gall-stone "cures" and which has been before the medical public the past few years, consists of three kinds of tablets: No. 1, calomel and padophyllin; No. 2, calomel, and No. 3, calomel padophyllin, camphor and menthol, none of which so far as is known have any specific action upon diseases of the biliary apparatus.

Bile Stasis—Naunyn, after a lifelong study of this subject, has recently called attention to the presence of micro-organisms, particularly the Bacterium Coli, in the duodenal portion of the common duct of healthy individuals. Naunyn calls this "normal bactericholia." So long as the stream of bile suffers no obstruction to its passage, this organism remains harmless. Whenever stagnation occurs for a while, a dangerous accumulation of bacilli occurs and this leads to bacterial infection of the bile. It is not necessary that the bile stream should be completely interrupted; any degree of stagnation may be sufficient. Mayer also suggests the importance of stasis by the assertion that gall-stones usually produce no symptoms so long as there is normal flow of bile. The importance of the prevention of bile stasis becomes therefore an indication of importance and it would appear that medical treatment has one of its best weapons in the ability to favorably influence bile flow in gall-bladder diseases.

Of exceptional importance and influence upon the flow of bile is the action of the choledocho-duodenal sphincter, that powerful constricting muscle in the pars intestinalis of the ductus choledochus. Thanks chiefly to the work of Pawlow; this sphincter is known to open only at definite times, chiefly during digestion; when closed it opposes a powerful obstruction to the bile so that the latter is forced to flow toward the place of less resistance—through the cystic duct into the gall-bladder. At the beginning of digestion, as soon as the sphincter is opened, the thickened bile from the gall-bladder mixes with the bile secreted by the liver and reaches the duodenum. Since in gall-bladder disease, the bile stagnates in the gall-bladder more than under normal conditions, it becomes more viscid and thick and is, therefore, hindered in its outflow during the periodic opening of the sphincter. One of the most important indications for medical treatment is, therefore, to promote its expulsion into the intestine.

Nothnagle years since made the assertion, based upon wide experience, that a full meal is the best cholagog. The explanation of the function of the choledocho-duodenal sphincter justifies Nothnagle's clinical observation. The periodic opening of this sphincter is essential to the entry of bile into the intestine. The more frequently the tone of this muscle relaxes, the more the outflow of bile is rendered possible. When the stomach is empty, or after it has discharged its contents completely, the outflow of bile into the intestines ceases. On this account, in gall-bladder disease, it is advisable to allow as short pauses as possible in the passage of food into the duodenum. In other words, patients should be given food as often as possible. A heavy meal undoubtedly stimulates bile production more powerfully than smaller meals. Increased flow of bile, however, is the object of treatment rather than increased production of bile. Mayer, writing on this subject, recently stated that the ordering of frequent but small meals forms the first principal in the dietic treatment of gall-bladder disease. The diet should be divided into at least five meals daily. All foods difficult of digestion and easily decomposed and lead to fermentation, should be strictly forbidden, and in all individual cases the diet must be adapted to the existing conditions of the stomach and intestines. This summarizes the whole question of the dietetic treatment. There is no special gall-bladder diet.

Other measures that promote the expulsion of bile from the gall-bladder and thus lessen stasis are the maintenance of normal intestinal peristalsis, the action of the abdominal walls and the pressure of the diaphragm upon the liver during inspiration, and exercise. The powerful influence of intestinal peristalsis which is communicated to the muscular apparatus of the biliary tract is suggested by the not infrequent occurrence of gall-stone colic following excessive purgation. It is important, therefore, to insure regular and normal intestinal peristalsis by dietetic measures and the use of mild laxatives. To its favorable influence upon intestinal peristalsis and to the removal of injurious products of digestion, combined with the dilution effect of free water drinking, is to be attributed the benefits obtained by the Carlsbad and similar treatments.

Those factors which exert an influence upon the respiratory action of the diaphragm and through it on the outflow of bile, such as deep breathing, physical exercise and the prohibition of badly fitting corsets, are of the greatest importance.

Cholagogs—The cholagog principle of gall-bladder disease is one of the oldest efforts of treatment. Numerous agents have been used with the idea to increase the amount of bile. Were it possible by medical measures to increase the secretory functions of the liver, it is questionable if the resulting increase in the amount of bile would prove beneficial. In certain complications, such as chronic obstruction of the common duct, this would be positively dangerous. The point is usually overlooked that it is not that bile is formed in insufficient amount, but that the bile which is formed, stagnates, and it is to the latter that treatment should be directed. Furthermore, in view of the action of the common duct sphincter, it is doubtful whether cholagog agents could, by the formation of increased amount of bile, sufficiently raise the pressure in the bile passages to overcome the resistance of the sphincter. This normally opposes a resistance corresponding to a pressure of about 700 m.m. of water, while the normal secretion pressure is equal only to about 200 m. m. of

water. It seems, therefore, that the use of cholagog agents even if effective would be irrational. It is, however, possible to produce a more easy outflow of bile into the intestine if we make the bile more fluid. The most effective means we have of accomplishing this is by drinking an abundance of water, especially upon an empty stomach. It should be taken as hot as possible, for cold liquids, especially in cholelithiasis, frequently induce attacks of colic, and because of the beneficial relaxing effect that heat exerts upon muscle spasm. Patients should drink one or two tumblers of hot water an hour before breakfast and a tumbler or two in the evening before retiring, and in small quantities frequently during the day.

While the control of stasis and infection are the logically defined indications in medical treatment according to the present limit of knowledge, it is probably far from the truth to conclude that they will ultimately be the only ones. Cholesterin metabolism, of which but little is definitely known; the influence of disturbances of the liver and of general metabolism upon fluctuations in the composition of bile; the various factors that control the excretion of bile, and the problems attending infections of the gall-bladder and immunity in acute infectious diseases are additional factors which, when worked out and put upon a clinical basis, may have a marked and perchance a revolutionary influence upon our conception of treatment.

Finally it should be said that perhaps in no other abdominal condition is complete rest more indicated than following acute manifestations of gall-bladder disease. Following acute cholecystitis and even after gall-stone colic, it is well to keep the patient in bed for several days after each attack, and after all inflammatory manifestations have passed off, and until no tenderness on pressure over the gall-bladder remains, a period which may extend from days to weeks, according to the severity of the case. By more careful insistence upon rest, chronicity of the disease may frequently be avoided.

THE TREATMENT OF TUBERCULOSIS WITH A SOLUBLE VACCINE—A PRELIMINARY REPORT.

By J. O. HIRSCHFELDER, M. D.

In the Journal of the American Medical Association of October 12, 1912, and April 5, 1913, a method of treatment of pneumococcus and of gonococcus infections with extracts of these microorganisms was described. Since these publications a comparatively large number of cases have been treated with equally favorable results. In addition similar favorable observations have been made with pancreatic extracts of the diplostreptococcus of Poynton and Payne and the viridans in acute endocarditis and in rheumatism. In staphylococcus infection similar results have been reported from the use of a pancreatic extract of that germ.

For about a year and a half experiments have been conducted in the hope of obtaining an extract of the endobody of the bacillus tuberculosis. Such a substance was derived from the digestion of the bacillus with pepsine. The living bacillus was treated with acidulated pepsine at 38 degrees, the action of the ferment stopped with alkalie, and the solution filtered through a Pasteur filter. It was found that pancreatine did not work quite as satisfactorily.

The extract was repeatedly standardized upon tubercular guinea pigs, and after an absolutely reliable preparation had been made and the method perfected so that the dosage could be accurately determined, a number of cases of tuberculosis were treated. The results have been very encouraging, but the number of cases has been too small and the duration of the observation too short to permit more than this provisional announcement of the method for the present. In none of the cases treated were any unfavorable effects noted. Rapid improvement has been observed both subjectively and objectively, the X-ray plate showing the clearing up of the tubercular deposits in the lungs. Several cases with tubercular laryngeal ulcers have been observed, and in all of them rapid healing of the ulcers has been noted by the laryngologist. Bone tuberculosis of years' standing has been seen to improve, and the progress has been recorded by the radiograph.

275 Post St.

MEDICAL ENDOWMENT FOR THE UNIVERSITY OF CALIFORNIA.

President Wheeler announced at Commencement Day, May 14, 1913, the George Williams Hooper Medical endowment for Medical Research. This is the greatest single, private gift ever made in California for the service of mankind. The gift is from Mrs. George Williams Hooper in memory of her husband. It consists of a foundation for medical research in connection with the University of California, and under the charge and ownership of the Regents of the University. The gift is in the form of property worth considerably more than $1,000,000, and will yield an income of $50,000 a year at present. None of the income is to be used for building purposes.

The institution will have an advisory board. In this board will be Mr. Pritchitt, President of the Carnegie Foundation; Wm. H. Welch, Professor of Pathology in the Johns Hopkins Medical School; Benjamin Ide Wheeler, President of the University of California; Dr. H. C. Moffitt, Dean of the University of California Medical School; Mr. E. H. Connelly, representing the interests of Mrs. Hooper, and two other persons to be chosen.

Announcement was also made of gifts aggregating $479,250 for a new hospital for the University of California Medical School. Work will begin on this building as soon as $600,000 is raised. As soon as the hospital is finished, the teaching departments of the first two years, which are now located in Berkeley, will be moved to San Francisco, thus bringing all the medical teaching to that city.

WALLACE I. TERRY, Acting Dean.

WHY DOES CANCER ATTACK THE FACE.*

By ANSTRUTHER DAVIDSON, M. D., Los Angeles.

Cancerous affections of the face are generally grouped into two classes: the ordinary squamous celled form usually found on the lower lip, and the basal celled or superficial form which for the purpose of this discussion may all be classed under the general term rodent ulcer. As the various authorities differ considerably as to the relative importance of the pathological findings, I feel less hesitation in grouping them in this manner.

Why rodent ulcer should be more common on the face than anywhere else has long been a subject of discussion, and on the general causes most authorities are in accord, but no very rational explanation has been forthcoming as to why rodent ulcer attacks most frequently certain parts of the face. As I have a theory as regards this, I wish to try it out on my fellow members and have their opinion on the subject.

Irritation of any kind is the prime factor in the production of cancer on the skin or anywhere else. The liability of X-rays and chemicals of the hydrocarbon series to produce rodent ulcer is well known. In the valley of Kashmir the natives are liable to cancer of the abdominal wall on account of their wearing the Kangri basket next to the skin, as a means of attaining artificial warmth.

Exposure to the sun and elements, particularly the sun, is an undoubted cause; the majority of our cases here are found in those individuals engaged in out-of-door occupations. Unna used to consider seamen were liable to a particular form of facial cancer.

A few years ago I saw in one of the Chicago clinics a man with five rodent ulcers on the right side of his face. Such a markedly unilateral development naturally suggested a special cause. The man, now 60 years of age, had been all his life a locomotive engineer and the ulcers were probably wholly due to the exposure of the right side of his face to the elements, and the irritating products of the smoke from the coal consuming engine.

Of all external sources of irritation, probably the sun is the most potent. Here in California it seems to me the number of rodent ulcers seen are entirely out of the normal proportion to all other skin diseases. Our climate is somewhat different from most of the States in the Union, and some general diseases common elsewhere are rare with us. Acute straight bronchitis, for example, is almost unknown here.

In skin diseases at present statistics give no reliable data. In rodent ulcer this is particularly so, as so many of its subjects are recent immigrants to the state that proportional statistics give us no data that would show the influence of the sun in the production of cancer of the skin.

While we concede, then, that the sun's rays are the exciting cause of rodent ulcer, there are other and more important factors underlying, for while we are almost all equally exposed to the sun's rays, but a comparatively small proportion suffer from cancer.

Here is a diagram on which I have marked the point of origin of the last 100 cases I have seen. Now draw a line across the nose at the junction of the lip and nose, and note that 94% are above that line. Then draw a line parallel thereto across the middle of the nose where the shadow of the ordinary hat ends, and 78% are above that line. Note then that the part of the face best protected from the sun is most subject to rodent ulcer; so that the solar irritation theory does not fully explain the peculiar distribution of cancer on the face.

Let us look on the subject from the other standpoint, the type of individual liable to cancer. As a general rule rodent ulcer is mostly found on individuals of a spare habit of body. Men of spare habit, though seemingly strong and wiry, are almost invariably the subject of some form of digestive disturbance, or subject to some disease that affects the metabolism of their food; they are all sufferers from mal-assimilation in some form or other. Of these mal-assimilants there are two well-marked types, the "rough neck" and the "atrophic skin." The "atrophic skin" presents a depigmented pale or bluish look and not infrequently closely resembles the skin that has been affected by leucoderma. This condition of the skin is most frequently associated with seborrhea of the scalp and thinning of the hair. Pathologically the whole skin is thinned and the subcutaneous fat reduced. In the "rough neck" type the man, once stout, has become thinner. The true skin, originally coarse, remains almost unaltered, but the subcutaneous fat has so diminished that the now overabundant covering in accommodating itself forms heavy folds and creases. You scarcely ever see rodent ulcer on the full blooded and empurpled face, or on those with a comfortably lined circumference.

Of those rodent ulcers depicted in the diagram shown, 95% were in individuals of a spare type of body.

These general remarks simply lead up to the point I wish to emphasize in this discussion.

Look again at this diagram and observe that certain regions are very frequently affected, and some are almost exempt. The region of the beard even in these days of smooth faces is practically exempt, the cheeks except on the malar process nearly so. The upper eyelid is free. I have never seen cancer on the upper eyelid, while the lower of apparently the same texture is quite frequently attacked. That is a fact curious and pertinent. The rim of the ear is not infrequently affected, 6% of those seen by me were in this situation.

For this peculiar distribution a curious theory was put forward by Dr. Evans of London many years ago. He attributed the partiality of cancer for certain localities to the presence of abortive lachrymal glands upon the parts of the face corresponding to the sites in which these glands exist in the higher mammalia, sheep, deer, etc. These glands are highly differentiated glands of an acinous or glandular type, and fetal residues in man may probably exist. This theory is a more definite adaptation of the "embryonic rest" theory that has

* Paper read before Los Angeles County Medical Society, March 20, 1913.

been applied to cancer in general. The distribution of these lachrymal glands correspond quite closely to the distribution of rodent ulcer on the face.

My theory is that those regions are most subject to rodent ulcer that are least mobile, and consequently least vitally nourished. The growth of the beard or the use of the razor stimulates the functional activity of these parts. The cheeks are active in mastication; the upper lid does practically all the dusting of the eyeball. The cutaneous structures at the corner of the eye, the nose itself, the ears and malar prominences have no underlying muscles, no mobility, no stimulation to activity. If the abortive gland theory were correct both sexes ought to be equally affected, but according to my figures only 12% were found in women. This disparity between the sexes is to be accounted for in part by the greater care expended on the face by women. The copious use of facial creams, frequent massage, etc., maintain a greater vitality in the skin. Then, too, women are endowed with an extra layer of adipose that still further maintains the tonicity of the skin.

The more degenerate, or the less vital the structure, the more the liability to cancer. In the general atrophy of the skin incidental to old age, those places devoid of all subcutaneous mobility are most liable to rodent ulcer. That seems to me the most plausible explanation of the peculiar distribution of facial cancer.

EVOLUTION IN THE STUDY OF THE HEART: A SURVEY.*

By HARRY I. WIEL, M. D., San Francisco.

Privilege indeed it is to live in a renaissance period. When we stop to consider how for centuries medicine has stood ineffectual in the face of the morbid heart, groping here and there for light in the midst of darkest ignorance and unable to attain any efficiency in this field, it is a great satisfaction to know that our own times have finally opened up new paths to knowledge and treatment of chronic heart disease. Dealing with the history and evolution of this matter, tracing the various steps by which this decidedly advanced and new knowledge has been attained, has proved a study of great fascination to the writer and it may be that the readers of this sketch will experience like enthusiasm in following this evolution with him, from ancient days to now.

Concerning the earliest years there is little to note in the way of progress. The views of Hippocrates, Aristotle and Galen held sway for centuries. These recognized the heart, arteries and veins and included in the system we now know as circulatory, the liver. There were supposedly two bloods, the natural and vital, in two closed systems, the veins and arteries. The liver was regarded as the central organ of the venous system, in which chyle was converted into blood, and from which it was distributed by the veins to the various parts of the body for nourishment. The structure of the heart and the direction in which the blood passed was known, but the chief function of the organ resided in the left ventricle where the "vital spirit" was created, a mixture of inspired air and blood. By alternate dilatation and collapse of the arteries, this mixture was kept in constant motion. Allowance was made in this scheme for a small amount of blood going from the right side of the heart to the lungs for their sustenance, and thence back to the left side of the heart. There was, however, no conception at all definite of the pulmonary circulation as such; in fact the main communication between the two sides of the heart was supposed to be by means of pores between the meshes of the interventricular septum.

Such in outline was the accepted idea of the cardio-vascular system which prevailed for centuries. It remained for that extremely fascinating character in the history of medicine, Michael Servetus of Villanova, Spanish born, theologian and scientist of continental Europe, the valiant and splendid heretic, contemporary of Vesalius, to make the first enlightened contribution. Sir William Osler, in a masterly biographical sketch of the man, has brought together with inimitable excellence, our knowledge of Servetus, and this society is indebted to the Regius Professor of Medicine in Oxford, for a reprint of that portion of the fifth book of "Christianissimi Restitutio" which contains the important scientific work of Servetus.

In this theological work, Servetus, in a few paragraphs, gives the now noted description of the lesser circulation. These facts he most probably discovered from dissection in the anatomical laboratory, for there is no record of any such thing in this affair as observation from the living organism. To Servetus himself, and to his contemporaries, the matter seemed of little import, and as far as clearing our ignorance in the problem of the circulation, it surely was of little significance. Literature does not remark much stir in scientific circles following this discovery. A certain Colombo a few years later announced the same observations with the addition of noting that the blood became mixed with the inspired air in the lungs and not in the heart, but all these things caused no more commotion than the contribution of Servetus himself. The influence of Galenical teachings was still rampant, and Servetus's work was allowed to remain hidden until after the publication of the greater work of William Harvey, nearly 100 years later. Charles Bernard, a surgeon of St. Bartholomew's Hospital in London, called the attention of William Wotton in 1697 to the important work of Servetus which had so long lain obscure, and Wotton in turn gave it to the world with the emphasis it deserved.

Had the discovery of the lesser circulation been heralded, one might think that it paved the way for the epoch making investigations which were to come, but we are justified in saying that until the time of Harvey, scientific investigation of the heart and blood vessels was a blank. Outside of the counting of the pulse in the arteries as early as 1464 by one Cardinal Nicolaus Cusanus using a *Wasseruhr*, we have little or no record of observa-

* Read before the general meeting of the San Francisco County Medical Society, Jan. 14, 1913.

tions of value upon the cardio-vascular system in motion. Until the advent of Harvey the Galenical views that distole was the active part of the cardiac cycle, systole being merely a rebound, prevailed, and therefore it is more than remarkable genius that was shown in the discovery of the circulation. This achievement in 1628 is not only noteworthy in the facts elicited, but is of supreme interest from the point of view of the subject this paper is considering. Calling dissection the first step in the evolution of the study of the heart, Harvey was the first working in this field to use vivisection, and so gave us a new means of research in this domain.

We have noted how Harvey, with his path shrouded in the darkness of ignorance, lighted a beacon illuminating the way for all future investigations. His genius and the worth of his work have been given fulsome but well merited praise by the world and need no further comment. Still, to emphasize their value from the standpoint of our theme, we must again point out with stress that Harvey brought to cardiac science new means for investigation, first vivisection, and second injection methods, for it was by the latter that he demonstrated the capillaries.

Harvey having cleared up the matter of the circulation and having shown that the heart contracts in systole and relaxes in diastole, we look for the next definite progress in the study of the myriads of questions which would naturally arise. Before we come to that and mark it well, we are led into a bypath of tremendous interest, a line of thought which was the first result of Harvey's work. We refer to the famous and still continuing discussion on the neurogenic and myogenic theories of the heart beat. He might well be called the real formulator of the myogenic theory, i. e., that the heart beats because of certain properties inherent in the heart muscle itself, but in the same century Willis formulated the neurogenic theory. This latter investigator postulated that the organ beat because of impulse conveyed to it from the cerebellum via the vagus. Haller's publications in 1757 were important in that they enunciated the myogenic theory as it is largely held to-day, i. e., that inherent irritability of the cardiac musculature is responsible for the heart's contraction and this irritability is replenished by the constant inflow of venous blood. Legallois in 1812 formulated the second neurogenic theory, to the effect that the cardiac impulse originated in the spinal cord and entered the heart via the sympathetic nerve. Bichat in the middle of the last century held that a ganglionic system governed all the viscera, and Remak's discovery in 1844 of inherent ganglion cells in the heart, lent great strength to this theory. Bichat's views held forty years until Gaskell in 1881-1883 published his convincing and remarkable experiments on the hearts of the frog and tortoise, which showed certain inherent functions of the heart muscle and proved that the ganglia represented merely an inhibitory apparatus.

Temporarily, we must leave these theories as far as we have brought them. They hardly mark, until the time of Gaskell, the epochs we are attempting to trace, and the arguments they brought forth were characterized for the most part by philosophizing rather than by advances in the methods of the study of the problem, and it is after all, with that, that we are at present most concerned.

To return to 1628, the date of Harvey's discovery, we search from there on for further disclosures in either facts or methods. We find nothing of tremendous importance until we come to Auenbrugger nearly a century later. True, Stephan Hales had introduced a sphygmascope in 1683, but that represented a small matter in comparison with Auenbrugger bringing percussion into use in 1722. This was perfected by Corvisart in 1809, the Napoleonic physician, and since then we know many names in this connection, notably, Skoda, Wintrich, Friedrich and Gerhardt.

This brings us to Laennec and the year 1819, and the part he played in the study of the heart is indirectly almost as great as that of Harvey, but alas not so beneficial. His introduction of the stethoscope and the all important emphasis medicine has since placed upon it, have marked one of the most if not the most splendid mistake in medicine. The mystery of sound, and at the same time its fascination, turned the attention of the best thought of that and later periods to the sounds of the heart and their modifications as the be-all and end-all in the study of the heart and its diseases. A great school of admirable and artistic cardiac clinicians grew and thrived under this conception, the school of murmurs they might be aptly termed. By the aid of auscultation, much of the hitherto obscure physical dynamics of the organ came to light, the causation of the heart sounds themselves became clear, and murmurs assumed a lucid significance.

The unfortunate feature of these things, however, was that, though they did open up new avenues for the diagnosis of the diseased heart, these avenues were actually blind alleys; and though they did give us a view, they brought us into little closer contact. In fact the real trouble was that they gave us a view but no real acquaintance; they introduced us to the morbid anatomy and we became enabled to say that such and such a valve was insufficient or stenosed, but came hardly into more intimate acquaintance with the pathological physiology of the organ, which knowledge is so essential to the correct understanding and intelligent management of its diseases. Spurred on by these ideas flourished so many noted men—Broadbent, Gibson, Friedrich, Austin Flint, names selected at random but typifying the class of heart clinician now passing. These men left us with little greater efficiency than we had before their time and it remained for our own century still so young, finally to put the study of heart disease on a basis actually fruitful and giving golden promise for the future.

Properly to understand this indispensable work of the last quarter century, which has done more than all the work of the aeons before, to give us real knowledge, it is imperative to glance at the development of the recent anatomical and physiological researches of striking importance. Stannius, Gaskell and Englemann in the '80's showed that

the beat of the mammalian heart originates in the sinus venosus. Stanley Kent of Oxford in 1892 gave the first description, however imperfect, of the muscular connection between auricle and ventricle. His Jr. of Berlin in the next year gave a more perfect account of the same structure which has since born his name, but Tawara, working under Aschoff in 1908 gave the most complete description we have and at the same time described the auricular-ventricular node which we know under his name and which shortly was to play such an interesting role in the explanation of certain cardiac rhythms. The year previous to this, Keith of the Royal College of Surgeons, London, working in collaboration with Flack, recognized at the junction of the superior vena cava and right auricle another node, the sino-auricular representing remnants of the primitive cardiac tube. This node was also destined to a part of importance, for it was here in the last two years that it was found that the normal rhythm of the heart receives its impulse, and this region was named by Lewis the pace-maker of the heart. As early as 1856 Köl-liker and Müller had demonstrated a current of electricity in the organ by placing a nerve muscle preparation in contact with the beating heart, and Waller in 1889 studied these electrical conditions and recorded them accurately by means of photographing the movements of the mercury meniscus of a capillary electrometer. The climax of these studies found its expression in 1903 when Einthoven of Leyden brought the string galvanometer into practical use for these purposes and gave us the electrocardiograph. This instrument and method of investigation has proved not only efficient but indispensable in clearing up many obscurities, and without its aid the discovery of auricular fibrillation, the greatest recent single advance of all, would not have been accomplished.

This, however, brings us a little ahead of our story and before going further we must turn our attention to the modern master who made this progress possible. The comparatively small English manufacturing town of Burnley in the last decade of the last century contained as its leading practitioner James Mackenzie, a thorough going Scot passed middle life and of so endearing a personality that the inhabitants not only looked to him for their medical welfare but their moral and social as well. He himself has said that he knew his patients and their families so well, most of them of the working class, that he entered their houses as often through the back door and kitchen as by the front entrance. His practice, as he records it, numbered as many as 3,000 patients; in fact he was so adored in the town that every one there refused to die without first seeing Mackenzie. The character of the practice was general, in fact there are some enlightening neurological observations of his on record, made upon surgical cases. Picture a man so occupied with medical routine finding the time and having the mental capacity for epoch-making observations and publications in the midst of it all, and the marvel of it becomes overwhelming.

In 1893-4 he published his first papers on the venous pulse, and these papers with the many that followed gave the impetus to the graphic method of the study of heart disease, which led to a field of knowledge of limitless extent, hitherto unexploited. True, Lancisi and Morgagni in the eighteenth century had made slight reference to the venous pulse and in 1794 Hunter had described it in the veins of the dog, but clinical thinkers had ascribed little moment to it, and it remained for the master mind of Mackenzie to realize what an important weapon the study and recording simultaneously of the venous and arterial pulses is in the clinical aspect of the heart. It was not the mere study of the pulses or the elaboration of a graphic method that actuated Mackenzie. The greatness of it lies in the newer and finer attitude he brought by these means to the study of the heart. Murmurs and heart sounds, though fascinating, helped us little in estimating cardiac possibilities for work under either normal or diseased conditions. Mackenzie gave us the conception of considering the organ from the standpoint of how much work it can do. To calculate this one must have actual knowledge of what the heart is doing and what it does under changed conditions. He found this knowledge could be attained through the graphic method, and so thrusting aside murmurs and valves for the time being, he turned his attention to the myocardium, and found that there lay the crux of the whole situation. In brief, it may be said that Mackenzie was the first to conceive that all dealings with the heart must be thought of and expressed in terms of the myocardium and its power for work, or to put it more clearly, he found that the pathological physiology of the heart contained the problems to be studied.

Working along those lines, he and those inspired by him attacked the arrythmias. Until then an irregular heart was called an irregular heart and nothing further, but Mackenzie discovered that the arrythmias are varied and protean in type. These different types had hitherto been effectually concealed, like a gopher in a hole, but Mackenzie smudged them out, and now we know the sinus or juvenile type, the paroxysmal tachycardia, auricular and ventricular, the extra-systole, the heart block, the bradycardias, the pulsus alternans, and other types. We are able to differentiate the serious ones from those of little import, and this only by the graphic method.

We now approach the extremely important discovery of auricular fibrillation, an outgrowth of Mackenzie's work. The pulse accompanying so-called decompensated cases of mitral disease, the "mitral pulse" had long been known and was early described by Marey, Riegel, Sommerbrodt and others. In later years Hering gave it the title "pulsus irregularis perpetuus," a most satisfactory and self-explanatory title. Mackenzie in taking records of these cases noted that the venous pulse showed a systolic movement and termed this the ventricular form of venous pulse. The presystolic or auricular movement being absent, he argued on negative evidence, that the auricle was paralyzed and the ventricle was assuming an irregular and independent rhythm. Later on he dis-

covered evidence that the auricle was in some sort of motion and so then he formulated his theory of "nodal rhythm." This held that if the auricle is beating but there is no separate auricular wave in the venous pulse, it must then be beating at the same time with the ventricle, and to do that it must be responding to impulses originating in the auricular-ventricular node. This idea prevailed for some years until Thomas Lewis, a pupil of Mackenzie of almost superhuman experimental ingenuity working in the University College, London, addressed himself to the subject.

In the course of experiments along other cardiac lines he noted, as others had, that electric stimulation of the auricles at a certain stage in the amount of stimulation used, caused them to enter a state of fibrillation and that during this fibrillation, the ventricles assumed a rhythm and gave a peripheral pulse record exactly similar to the "pulsus irregularis perpetuus" in man. The electrocardiograph being now at hand, it occurred to him to sew electrodes into the auricle, sew up the chest and take electrocardiograms of the animal in which auricular fibrillation could be produced at will. These electrocardiograms he compared with those of patients having the "mitral pulse" or "pulsus irregularis perpetuus," and found them identical. Mackenzie's idea of nodal rhythm had to give way to the proven fact of auricular fibrillation, and Mackenzie himself was the first to recognize and accept it.

The significance of this discovery was tremendous. Hirschfelder in Baltimore a little over a year previously had mentioned auricular fibrillation in connection with paroxysmal tachycardia but his ideas later were shown to be erroneous and he had not at all the conception of the true relationship of auricular fibrillation to clinical cases. Rothberger and Winterberg of Vienna one year before (1909) had hit upon the phenomenon independently, but they had not given it as elaborate experimentation and detailed complete proof, and the achievement will go down in medical history as Lewis's work.

This threw a new light on our previously termed "decompensated" hearts and for the first time in medical history we knew just what was happening in such cases. Naturally the first question presenting itself would be, how does that help us if at all? Is there any more we can do for these patients now than we could before, just because we know what their auricles were fibrillating? This very question occupied Mackenzie and particularly one co-worker, Cushny. They turned their attention to the therapeutics of this affection and soon found that an old drug, differently used than hitherto, namely digitalis, became an all powerful weapon. I say differently used, meaning used from the standpoint of the myocardium. Cushny, bearing the properties of the myocardium in mind discovered that digitalis had a selective action upon conductivity, delaying that function and thus slowing the ventricle by blocking many of the myriads of impulses arising in the fibrillating auricle. Infinitely better results than heretofore were obtained, by checking the use of the drug by the graphic method in each individual case. This and related phenomena led Cushny and Mackenzie to study the drug anew and resulted in an infinitely clearer understanding and fuller knowledge of the use of what is probably the main therapeutic agent we have in heart disease. Their publications are extremely recent, the last but a little over a month back, but the detail of cardiac therapy lies without the scope of this paper and will be considered elsewhere at a later date.

It must not be inferred that it is the writer's idea that Mackenzie was the first to use the recording of the pulse in the study of the heart. Ludwig, in 1847, working in the physiological field composed the kymograph for the laboratory. Hering and Hurthle later improved on it and brought it to its present effective state as a physiological instrument. Ludwig studied the velocity of the blood by means of the *stromuhr* in 1867 but it was really Vierordt in 1855 who first applied the graphic method to the study of the pulse. Marey in 1860 devised the sphygmograph and a few years later Dudgeon gave us the improved and now familiar instrument. The physiological laboratories had for years recorded the pulse and heart motions by means of manometers, tambours and cannulae, but it remained for Mackenzie to correlate these methods and give them a working application in the clinical study of the human heart.

Another important and earlier outgrowth of the graphic method is seen in the clarification of our knowledge of the condition we learned by these means to know as heartblock. Clinically, paroxysmal bradycardia was known as far back as 1761 when Morgagni described a case of "epilepsy with slow pulse." The first clear clinical description fell to the lot of Robert Adams of Dublin in 1827 who published one case. William Stokes, a fellow Irishman, published clear accounts of 4 cases 20 years after, and it is from the names of these physicians that the title Stokes-Adams disease or syndrome is derived. The fact that in this condition, auricle and ventricle were beating at different rates was first noted by Galabin of Guy's Hospital in 1875, who came to this observation through means of auscultation. Eight years later Tigerstedt and also Woolridge were successful in experimentally dissociating auricle and ventricle but their experiments did great damage to the whole heart and had little more than historic value. His in 1895, 2 years after his description of the bundle announced his experimental accomplishment of heart block but he published no tracings or specimens. Four years after this he described, as well as Wenkebach, human cases of heart block but Chauveau in 1885 had already done so in a rather primitive manner. Mackenzie in the years 1902-5 published the records of several cases and advanced our knowledge of the recognition of this condition vastly, but it remained for Erlanger in Johns Hopkins University, in a series of experiments as ingenious as those of Lewis on auricular fibrillation, to disclose to the world all the obscurities which had hitherto been connected with the subject. It is now well known how by means of a specially devised clamp, he was able, by com-

pressing the His bundle to any degree wished, to produce all degrees of heart block, from the mere dropping of a ventricular systole, to complete block. He did not confine his studies to experimental work alone, but by studying human cases at the same time, he found it possible to draw convincing analogies and conclusions which later autopsies from all parts of the world amply verified. These cases were naturally studied by the graphic method, in fact the information would have been obtainable by no other method. It remained for the electrocardiograph, whose records are accuracy beyond question, to confirm the whole matter.

The discovery of heart block and the interdependence of the contraction of auricle and ventricle, upon muscular connection seemed at once to be the last word in the proof of the hyogenic theory of the heart, which discussion we had left at the time of Gaskell's work. Further light, probably in support and confirmation of this theory will be found in some startling experiments now going on in this country. We refer to the work of Carrel in the Rockefeller Institute and his "visceral organism" which opens possibilities for the observation of the heart beating apart from the influences of the central nervous system.

The appurtenances of the graphic method did not stop with the sphygmograph, the polygraph and the electrocardiograph. As is often the case, the method has been driven to extremes, and innumerable refinements, modifications, and new instruments sprang up on every side, some of them ingenious but most of them leading into unproductive by-paths. Many workers along these lines became befogged and mistook the means for the end. A large group of physicians seized eagerly upon the new toys and spent much labor collecting tracings, but little labor upon their interpretation. For such, the more complicated polygraphs which were furnished had an irresistible fascination, for in sooth these instruments would give simultaneous tracings of the venous pulse, arterial pulse, apex beat and blood pressure, and ingenuity is developing to such a degree that it would not have been surprising had some machine been invented to do all these things and in addition supply music for the patient's entertainment and tell his fortune. Nevertheless there were some inventions of interest and mayhap of promise, notably Moritz's Orthodiagraph and Frank's apparatus by means of which heart sounds could be graphically recorded.

Pausing now at the distance to which we have trudged in all these years and looking back at the beginnings, we must realize the tremendous revolution the last 20 years have wrought for us. Harvey took us by the hand and led us out of superstition. Laennec was the siren who lured us from the straight and narrow path and tempted us into the land of fascination but little accomplishment; but Mackenzie was the deliverer who led us from out of the wilderness into the light. It was he who taught us that such diagnoses as mitral incompetency or aortic stenosis said a little, not much and certainly not all in the case of a heart suffering from chronic disease. It was he who conceived the idea that for us to be effective in the handling of diseased hearts, we must know what they are doing and can do, and he pointed out the means by which such knowledge is readily obtainable. Looking at heart disease from this point of view we can, as he emphasized in his Oliver Sharpey lectures of last year, get a rational basis on which to diagnose, prognose, and treat. His work, and all that followed under his inspiration, directly or indirectly, promises bright things for the future of our fight against heart failure, and even though we should not find the actual cures we hope for, we now at least have the satisfaction that we are approaching the problems rationally, and that after all is the most advanced aim for which we could wish.

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(The bibliography here submitted is not meant in any sense to be complete, but represents merely the main sources from which the material for this article, i. e., the historical material was obtained. Several of the references here given in themselves give complete bibliographies, notably the volume by Mackenzie and that by Lewis.)

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STEREO-ROENTGENOGRAPHY IN PULMONARY TUBERCULOSIS.*

A CLINICAL AND ANATOMICAL STUDY.

By WALTER W. BOARDMAN, M. D., San Francisco.

The question of the value of radiographic examination in the diagnosis of pulmonary tuberculosis is one which has called forth widely varying opinions. To investigate this question a complete stereo-roentgenographic apparatus was installed in the Phipps Dispensary of the Johns Hopkins Hospital and arrangements made for the study of a large series of cases, the radiographic findings being checked by careful clinical and when possible by autopsy examination, the work being carried on by Drs. Dunham, Wolman and myself.

As you may recall the X-ray is a form of radiant energy possessing the following important properties. The rays radiate in straight lines from their point of origin, they can neither be reflected nor refracted, they are capable of penetrating bodies opaque to ordinary light, in penetrating various materials the degree of absorption of the rays is directly proportional to the specific gravity of the materials, and finally the rays are capable of exciting photographic plates. We have, therefore, in the X-ray a means of recording differences of density occurring normally or abnormally in the tissues of the human body.

In a radiograph of the chest we have then a shadow picture in which the dense tissues or struc-

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tures appear as light areas, the less dense tissues or structures as darker areas. On the single radiographic plate of the chest we must of necessity have various shadows superimposed one upon another. This naturally increases the difficulty of careful study and exact interpretation of the individual shadows. In stereo-radiographs the chest may be seen in true perspective, the various shadows occupying the same relative position in the image as the structures which cast these shadows occupy in the chest.

This result is obtained by applying the principles of binocular vision, obtaining two radiographs corresponding to the right-eyed and left-eyed image respectively and viewing simultaneously, by means of the proper optical device, the right-eyed image with the right eye and the left-eyed image with the left eye. Various types of apparatus have been perfected for this work, the common principle in all being a movable plate holder and movable tube stand. With such an arrangement the patient is placed in position and the first exposure made. By some mechanical device an unexposed plate is quickly substituted for the exposed plate and simultaneously the X-ray tube is moved the necessary interpupillary distance and the second exposure made. This must all be accomplished without disturbance to the patient, and within the period of a sustained inspiration. For viewing the negatives some suitable stereoscope is used.

Examining a set of stereo-roentgenograms of a normal chest, one sees the shadow picture of the bony framework, with its covering of soft parts enclosing the chest except inferiorly, where the shadow of the diaphragm is apparent. The shadows within the chest seen in true perspective, may be divided into three main groups, the "heavy-central shadow" extending from the upper boundary of the chest cavity to the diaphragm; the "hilus shadow" radiating irregularly on either side of the center of the preceding shadow; and lastly, the finer markings in the lung fields.

The "heavy central shadow" is cast by the heart and pericardium, the aorta, the esophagus and the lymphatic and fibrous tissues of the mediastinum. The anatomical basis for the hilus shadow and the finer markings in the lung fields was still a fruitful subject for discussion, numerous investigators, Hickey, Holzknecht, De La Camp, Kienbock, Cowl, Rieder, Cummington and others, having expressed varying opinions based on more or less careful study. As Rieder had stated, although the matter was not definitely proven, the most generally accepted opinion was that the shadows in question were due in part to the blood vessels and in part to the bronchi. However, Fraenkel and Lorey in 1910 stated that "the anatomical substratum of the hilus shadow consists entirely of the blood vessel of the lungs and that under normal conditions the bronchial arborization gives no shadow on the Roentgen plate." Our investigations of this question fall into four divisions; the study of stereoscopic plates of normal and diseased lungs before death; the study of stereoscopic plates taken shortly after death, the radiographic and autopsy findings being carefully compared; the study of stereoscopic plates of animal and human lungs in which the blood vessels and bronchi had been injected with bismuth emulsions or other shadow casting materials; and finally, the study of plates of animal and human lungs in which the different structures had been dissected.

The study of stereo-roentgenograms of normal and diseased lungs clearly demonstrated that the hilus shadows and the shadows in the lung fields are due to structures originating at the root of the lung and radiating toward the periphery, as demonstrated by De La Camp. These shadows could be divided into groups corresponding to the anatomical division of the lung into lobes, but we were unable to say whether they were due to pulmonary blood vessels or bronchi or both.

The autopsy work, although of considerable interest in some respects added nothing to our knowledge of the subject in hand.

The conclusions reached by injecting animal and human lungs were disappointing, since bismuth injections of the bronchi seemed to demonstrate the bronchial origin of the shadows, whereas similar injections of the blood vessels were equally convincing of the blood vessel origin of the shadows. When both blood vessels and bronchi were simultaneously injected the resulting shadows were too dense to allow of careful study.

Radiographs of pigs' lungs showed the bronchi as dark bands. Partial injections of such lungs showed the artery lying along one side of the bronchus—the vein along the other side. The lobules overlying the bronchus were separated and a section of the bronchus was removed and placed on another portion of the lung. The plates showed a definite shadow cast by the removed bronchus and also definite shadows cast by the artery and vein in the space from which the bronchus had been removed. These latter shadows were less dense than the shadows with which they were continuous above and below. In other words the arteries and veins cast shadows in X-ray plates of pigs' lungs, but normally their shadows are augmented by the shadow cast by the wall of the bronchus.

Bearing these results in mind a normal human lung was obtained shortly after death and inflated to its natural degree. The radiographs presented linear shadows radiating from the hilus toward the periphery. The lung tissue over one of these shadows was carefully separated until a vessel was exposed throughout its entire extent from hilus to periphery. A second set of plates showed that the position occupied by the vein was that of the structure casting the original shadow. The vein was carefully removed and placed in another position on the lung. The third set of plates showed a shadow undoubtedly cast by the removed vein and a shadow still persisting in the original position. In removing the vein an artery had been exposed, this was now removed and placed parallel to the vein. The fourth set of plates showed definite shadows cast by the removed artery and vein and a faint shadow in the original position correspond-

ing to the position of the bronchus, which was removed and placed parallel to the artery and vein. The fifth set of plates showed shadows cast by the removed artery, vein and bronchus and an absence of the linear shadow in the original location. The proximal end of the bronchus cast a very decided shadow. A section of the primary bronchus and of the large thick-walled pulmonary artery were placed side by side on another portion of the lung. The sixth set of plates showed a very definite shadow cast by the primary bronchus and a much less marked shadow cast by the walls of the pulmonary artery. Finally two vessels were exposed and a small quantity of citrated blood injected into them. These plates showed slight but definite increase in the density of the shadow beyond the tip of the syringe needles.

From the experiments we may conclude that the walls of the arteries, the walls of the veins and the walls of the bronchi cast shadows in radiograms of normal lungs removed from the body, and that these structures with their blood and accompanying fibrous and lymphatic tissues are collectively responsible for the shadows at the hilus and in the lung fields.

Referring again to a stereoroentgenogram of the normal chest, we now know that the hilus shadow is cast by the primary branches of the pulmonary vessels with their contained blood, by the walls of the primary bronchi and by the lymphatic and fibrous tissue surrounding them. Normally the hilus shadow is of moderate density, irregular outline and small extent, merging internally with the heavy central shadow. From the outer irregular border dense shadows (the so-called heavy trunks) radiate toward the periphery. The normal appearance of the hilus shadow will be altered by any process which alters the size, position or density of the tissues casting this shadow.

The shadows in the lung fields may be divided into two groups, the heavy trunks and the fine linear markings. The heavy trunks are shadow bands radiating from the hilus and cast by the large bronchi and blood vessels supplying the different lobes of the lung. Three such trunks can usually be recognized on the right side, two on the left. The trunks to the lower lobes are denser than those to the upper. The fine linear markings are seen to originate from the heavy trunks and to radiate in fairly straight lines toward the periphery dividing and subdividing like the branches of a tree. These shadows are cast by the smaller vessels and bronchi which radiate from the main branches supplying the separate lobes. With our present technic we are unable to trace them to the periphery of the lung fields since the density of the terminal vessels and bronchi under normal conditions is not sufficient to record itself on the X-ray plate. Any condition altering the size, shape or density of these structures will alter the shadows cast by them upon the radiographic plate.

Stereo-roentgenographs of early but definite pulmonary tuberculosis show changes in the hilus shadows, the heavy trunks and especially in the fine linear markings. The alterations in the hilus shadow usually consist of an increase in size and density. Here and there scattered shadows of marked density are frequently noted, these resulting from involvement of the glands with or without calcification. The heavy trunks extending toward the involved area usually appear broader, denser and less regular in outline than in the normal. The fine linear markings in the involved area are also found to be broader, and less regular in outline. They are broken in continuity and spotted here and there with circular shadows. They also reach nearer the periphery than in the normal. As a result of these changes the markings instead of appearing as more or less straight linear shadows, seem to cross and interweave, producing a delicate meshwork. In the uninvolved areas of the lung the shadow picture is normal or but slightly altered. These changes, increase in the hilus shadow, increase in the shadow of the heavy trunks, together with the studding, the interweaving, the increase in density and breadth, the irregular outline and the extension of the linear markings to or near the periphery, constitute a stereo-radiographic picture which we believe to be characteristic of pulmonary tuberculosis.

With more advanced lesions the alterations become more marked and more readily recognized, the linear markings become more irregular and broken, the studdings larger and denser and the interweaving closer until eventually the whole area appears as a more or less homogeneous shadow due to the presence of gross areas of consolidation within the lung. The changes produced by the presence of cavities, pleurisy with effusion, etc., need not be considered at this time; nor shall I enter into the differential diagnosis stereo-radiographically of pulmonary tuberculosis and other pulmonary diseases, further than to state that such differentiation can be made at least in the vast majority of cases.

During the course of our investigation something over three hundred patients were examined, the clinical and stereo-radiographic findings being independently made and recorded. Then and not till then were the findings by the two methods compared. Only those patients examined clinically by Dr. S. J. Wolman are here considered. The series includes 92 cases. In the 92 cases there was disagreement in seven cases, in one disagreement was absolute, in six it was partial. The one case in question concerned a child of three years giving a positive tuberculin reaction but negative physical findings, the radiograph, however, discovered a small lesion in the left lung. The disagreement in the other six cases was concerned with the extent of the lesion rather than with its nature. Of the 85 cases in which the findings by the two methods agreed, 39 were clinically definite pulmonary tuberculosis, 6 of them being classed as fibroid. In all 39 cases the stereo-radiographic findings agreed with the clinical, and four of the number were classed as fibroid. Twenty-four cases gave definite physical signs and tuberculosis was suspected but could not be proven clinically; radiographically abnormalities corresponding to the region of the physical signs were noted, in twelve a diagnosis of tuberculosis was ventured, in the re-

maining twelve the radiographic diagnosis was "probably tuberculosis." Fourteen cases clinically diagnosed as normal were so reported radiographically. The remaining eight cases showed abnormalities in the lungs both clinically and radiographically, but were diagnosed as not tuberculosis by both methods.

In summing up the ninety-two cases here considered there was absolute disagreement in one case, partial disagreement in six and agreement in eighty-five cases. Of the eighty-five cases, thirty-nine were definitely tuberculosis, twenty-four probably tuberculosis, eight showed pulmonary involvement other than tuberculosis and fourteen cases were normal. On this showing it seems reasonable to conclude that the stereo-roentgenographic examination in the hands of a trained physician is capable of discovering pulmonary lesions and differentiating the tuberculous from the non-tuberculous as readily as the specialist by means of history, physical examination and tests. Therefore, there can be no question of the value of this method of examination to the specialist and especially to the general practitioner as an aid in the diagnosis of pulmonary conditions. However, it must be remembered that stereo-roentgenography is an *aid* and not a *means* of diagnosing pulmonary tuberculosis; the radiographic findings should in practice always be considered in conjunction with the clinical findings.

There yet remains a phase of the radiographic diagnosis of pulmonary tuberculosis to be considered. The alterations in the hilus, heavy trunks and fine linear markings just referred to are dependent upon morbid changes in the structures casting these shadows due to the local activity of the tubercle bacilli. Some authorities have attached diagnostic importance to certain alterations in the shadow of the heart, diaphragm and chest wall. Alterations in these structures, with the exception of the diaphragm, cannot depend upon the local activity of the infecting organism and must therefore be considered either as indicating a predisposition to the disease or as changes secondary to the disease. Of these conditions the "small pendulous heart" and the calcification of the rib cartilages have attracted most attention, others occasionally mentioned are narrow interspaces, contraction of one side of the thoracic wall, decrease in the angle made by the neck muscles with the clavicle, various abnormalities in the shape of the upper aperture of the chest cavity, alterations in the outline and height of the diaphragm, etc. In investigating these points 153 cases were examined in all of which the relative position of the plate, patient and X-ray tube was the same.

As a basis for comparison of individual cases or of groups of cases it is evident that the actual area of the cardiac shadow would be valueless as this must vary with the size, age, sex, etc., of the patient. In was, therefore, necessary to consider the size of the heart shadow in relation to the size of some other shadow, the original of which varies in the same way as does the heart with the size, age, sex, etc., of the patient. To this end the greatest transverse diameter of the heart shadow was compared with the greatest transverse diameter of the chest shadow and the resulting ratio, which therefore expresses the size of the heart in relation to the size of the chest, was termed the cardio-thoracic index.

Table I.

Showing the influence of Sex upon the Cardio-Thoracic Index.

		Cardio-Thoracic Index		
Sex	No. of Cases.	Average.	Max.	Min.
Male	47	0.446	0.52	0.35
Female	33	0.430	0.50	0.37

Table I shows that in the cases examined the average cardio-thoracic index, independent of age or physical condition, was slightly greater in the males than in the females.

Table II.

Showing the Influence of Age upon the Cardio-Thoracic Index.

		Cardio-Thoracic Index		
Age.	No. of Cases.	Average.	Max.	Min.
0-5 years	3	0.456	0.47	0.39
5-15 years	11	0.446	0.50	0.41
15-30 years	45	0.450	0.50	0.37
30-40 years	15	0.443	0.52	0.39
40-50 years	4	0.42	0.52	0.40
50-60 years	2	0.42	0.45	0.40

Table II shows the cardio-thoracic index, independent of sex or physical condition, to be practically constant in the different age periods, with a slight tendency to decrease in later life.

Table III.

Showing the Influence of Pulmonary Tuberculosis upon the Cardio-Thoracic Index.

Stages of		Cardio-Thoracic Index		
Disease	No. of Cases.	Average.	Max.	Min.
Neg.	14	0.452	0.50	0.35
Doubt	16	0.440	0.48	0.39
1st Stage	6	0.435	0.46	0.41
2nd Stage A	14	0.437	0.50	0.37
2nd Stage B	13	0.449	0.48	0.40
3rd Stage	17	0.438	0.52	0.39

Table III is of special importance. As will be seen, the cases are divided into non-tuberculous, doubtful and tuberculous. The tuberculous are subdivided into four groups depending on the stage of the disease, first stage, early second stage, late second stage and third stage. Here the cardio-thoracic index is practically constant in the different groups, with, however, a slight tendency for the average to be smaller in the tuberculous than in the non-tuberculous and doubtful cases. However, one fact must be borne in mind, and this is, that the cardio-thoracic index varies between fairly wide limits in the individual cases, both normal and tuberculous, as is shown by the maximum and minimum indices given in the various tables.

Table IV.

Showing the Influence of Cardio-Vascular Disease Upon the Cardio-Thoracic Index.

		Cardio-Thoracic Index		
Disease	No. of Cases.	Average.	Max.	Min.
Myocarditis	5	0.564	0.59	0.54
Mitral Insufficiency	7	0.502	0.57	0.43
Aneurism	6	0.543	0.58	0.52

Table IV shows a high cardio-thoracic index in 18 cases with definite cardiac lesions. These cases naturally are not included in the previous tables.

We know that a small heart is common in individuals suffering from chronic wasting disease, such as tuberculosis, carcinoma, etc. Here the small heart is merely an expression of the general wasting and is not characteristic of any special disease noma of the esophagus, the radiograph taken just process. Thus in one of our cases, dead of carcibefore the autopsy showed a cardio-thoracic index of 0.37. However, in our third-stage cases (Table III) which were all ambulatory, the average cardio-thoracic index was 0.438 with the maximum 0.52 and a minimum of 0.39. Evidently, then, the small heart is far from constant even in third-stage cases. Now as the value of the radiographic examination must depend upon one's ability to discover disease processes at a time when the clinician is still doubtful, or anxious for confirmation of his findings, it is useless to attempt to base a diagnosis upon changes which are neither constant nor characteristic, even in the late stage of the disease.

From the preceding we may safely conclude that the average cardio-thoracic index is practically constant in the different age periods, that it is slightly smaller in females than in males, and that in the tuberculous, although on the average it shows a very slight tendency to be somewhat smaller than in the non-tuberculous, this average tendency is so slight and varies so widely in the individual cases, that it cannot be considered a sign of any value in the radiographic diagnosis of pulmonary tuberculosis.

Regarding the occurrence and diagnostic value of calcification of the rib cartilages, two theories have been advanced. The one that calcification, by interfering with the free movement of the thoracic walls, renders proper aeration of the apices impossible and therefore acts as a strong predisposing factor to pulmonary tuberculosis, the other that the calcification is secondary to the pulmonary disease and depends upon altered metabolic processes. In Tables V-VII, the results of the investigation of 153 cases are tabulated. Cases showing complete calcification of the first cartilage, with or without calcification of other cartilages, are designated positive. Cases showing partial calcification of the first cartilage, with or without partial calcification of the other cartilages, are designated slight.

Table V.

Showing the Influence of Sex upon the Calcification of the Rib Cartilages.

		Calcification.		
Sex.	No. of Cases.	% +	% Slight.	% —
Male	85	30	22	48
Female	68	17	17	66

Table V shows that calcification was more common in the male cases, independent of age and physical condition.

Table VI.

Showing the Influence of Age upon the Calcification of the Rib Cartilages.

		Calcification.		
Age.	No. of Cases.	% +	% Slight.	% —
0— 5 years	6	0	0	100
5—15 years	20	0	0	100
15—30 years	78	19	17	64
30—40 years	39	33	33	34
40—50 years	9	40	47	13
50—60 years	9	68	32	0
60—70 years	2	50	50	0

Table VI shows that calcification ran parallel with the age.

Table VII.

Showing the Influence of Pulmonary Tuberculosis upon Calcification of the Rib Cartilages.

		Calcification.		
Stage of Disease.	No. of Cases.	% +	% Slight.	% —
Neg.	37	36	28	36
Doubt	35	10	27	63
I Stage	9	0	10	90
II Stage A	37	19	22	59
II Stage B	16	22	7	71
III Stage	19	46	5	54

Table VII shows that calcification was present in a large percentage of the non-tuberculous cases. It also shows that calcification was absent in the first stage of the disease and increased in frequency with advance in the disease; however, only in the third stage cases did the percentage of positive cases exceed that found in the non-tuberculous cases. It might be mentioned that the patients comprising the third-stage group were slightly older than those comprising the non-tuberculous group.

From these tables we may conclude that calcification of the rib cartilages, especially the first, is more common in males than in females, that it increases in frequency with advancing years, and finally, that its incidence in pulmonary tuberculosis is only accidental or a late secondary change, and that its occurrence is of no diagnostic significance whatever in the individual case.

Table VIII.

Showing the Absence of any Relation Between the Width of the 2d Interspace and Pulmonary Tuberculosis.

		Width of 2d Interspace.		
Disease.	No. of Cases.	Average.	Max.	Min.
Neg.	36	3.0 cm.	4.4 cm.	1.5 cm.
Doubt	35	2.7 cm.	4.0 cm.	1.8 cm.
I Stage	9	2.9 cm.	3.8 cm.	2.2 cm.
II Stage A	37	2.9 cm.	4.5 cm.	2.0 cm.
II Stage B	16	3.3 cm.	4.0 cm.	2.5 cm.
III Stage	20	2.8 cm.	4.0 cm.	1.8 cm.

Another sign to which attention is sometimes called is the presence of narrow interspaces in individuals suffering from pulmonary tuberculosis. The width of the 2d interspace in the left mid-clavicular line was measured and the results recorded (in Table VIII). From this it is seen that no apparent relation exists between the width

of this interspace and the presence of pulmonary infection. Whether we may take the width of the 2d interspace as a basis for comparison is, however, open to argument.

Table IX.

Showing the Absence of any Relation Between the Angle of the 6th Rib and Pulmonary Tuberculosis.

Stages of Disease.	No. of Cases.	Angle of the 6th Rib. Average.	Max.	Min.
Neg.	36	100°	104	85
Doubt	35	97°	103	99
I Stage	9	101°	105	97
II Stage A	37	99°	104	97
II Stage B	16	100°	103	80
III Stage	22	97°	109	85

No very absolute measurements could be made of the angle of the ribs, but in Table IX we have recorded the average angle made by the spine and the sixth rib on the left side. Apparently no relation exists between this angle and pulmonary tuberculosis.

The position in which our cases were radiographed made it impossible to study the shape of the upper aperture and alterations in the angles of the neck muscles.

No definite conclusions were reached from our study of the height of the diaphragm, since the average height was about the same in the tuberculous and non-tuberculous cases and the variation in individual cases was very great. There are, however, several interesting features about the diaphragm shadow, especially the occurrence of irregularities, which are worthy of more extended consideration.

In conclusion, I believe I am justified in saying:

1. That the small pendulous heart, calcified cartilages, narrow interspaces, and excessive sloping of the ribs, are not only valueless, but are absolutely misleading, if considered as positive signs in the radiographic diagnosis of pulmonary tuberculosis.

2. That the hilus shadows and the fine markings in the lung fields are composite shadows cast by the bronchi, by the blood vessels with their contained blood, and by the fibrous and lymphatic tissues accompanying these structures.

3. That pulmonary tuberculosis produces alterations in these shadows as seen in stereo-roentgenographs which we believe to be characteristic of this disease.

4. And finally, that there can be no doubt of the great value of stereo-roentgenography as an aid in the diagnosis of pulmonary tuberculosis and other pulmonary affections when considered in connection with the history and physical examination.

Discussion.

Dr. Rene Bine: I wish first to congratulate Dr. Boardman upon the excellence of his work. I think the members of the Academy should be ashamed of themselves not to turn out to a larger extent and learn something on this subject, which is more or less new to most of us in the community. I was much impressed with Dr. Boardman's statement that the experts at the Phipps Institute found that the signs obtained on physical examination agreed pretty thoroughly with the X-ray findings. I think the X-ray has been of great value to most of us, and has proven a great stimulus to the average practitioner to do very accurate work in physical diagnosis because of his being frequently checked up by the X-ray. These pictures are certainly beautiful, and I think that we should give them greater study than is possible at this demonstration. I should like to ask what is the expense of this method as compared with the old.

Dr. Addis: I should like to hear Dr. Boardman's experience in comparing the findings of averagely skilful physical examination with the results of X-ray examination. It has seemed to me that in the hands of those who have not had a very large experience in physical examination, or who have not made a very special study of it, the tendency is rather to exaggerate the importance of slight variations in the quality of percussion resonance and breath sounds than to miss any signs really indicative of anatomical changes, and that thus one of the most important benefits for the ordinary man of X-ray examination of the chest is not so much to show him what he has missed as to let him see how much he has found which does not exist at all.

Dr. Spaulding: I wish to ask Dr. Boardman if there would be any improvement in the study of deformities of the pelvis by this means. The X-ray has been proven worthless in this work, even after marking with pieces of metal.

Dr. Boardman: In reply to Dr. Bine's question regarding expense, I may say that two plates are necessary by this method, but frequently two or more plates are required by the old method—so the expense is practically the same.

Regarding Dr. Addis's question, I believe that the X-ray usually discovers more disease than the clinical examination would lead one to expect, even with the most expert examination.

Replying to Dr. Spaulding's question regarding the measurement of the pelvis by means of stereo-roentgenography, this method has proven satisfactory. A report of the method may be found in the American Quarterly of Roentgenography, April, 1911, by W. F. Manges of Philadelphia.

TWO CASES OF CEREBELLAR DISEASE FOLLOWED BY AUTOPSY.*

By WALTER F. SCHALLER, M. D., San Francisco.

I wish to present the history of two cases of cerebellar disease, both of which came to autopsy. They represent two different types in their symptomatology: one was of sudden onset with no tendency towards a progressive course and with no evidence of intracranial pressure, while the other came on slowly and progressively with marked signs of intracranial pressure. In these two cases I wish to lay special stress on the analysis of the character of the ataxia.

Case 1, a boy aged 4½ years, was brought to the Stanford clinic complaining of difficulty in walking, of dizziness, of suddenly falling to the ground without loss of consciousness and of headache. First entering the children's clinic he was referred to the neurological clinic. About three months previously he had a fall from a height of 6 feet, striking on his head. This was followed by a nosebleed and some fever. The mother believed that the trouble, which had become progressively worse up to the present time, dated from this fall. Nothing of importance was brought out in the family history or the past history.

* Read before the San Francisco County Medical Society.

The examination showed an average sized boy for his age, fairly well developed and nourished. The speech was definitely scanning in character. The child laughed rather easily and there was at times the suggestion of Zwangslachen. The mother stated that the child had been of rather serious nature prior to his illness and believed that she noticed a change in this respect. It was noticed that the child tired easily at play and the mother said that she believed that he did not see so well as formerly. The walk was staggering in character and Romberg's symptom was present. Tests for ataxia in the upper extremity such as the finger-nose test were performed with moderate precision; in fact voluntary movements were relatively little affected as compared with the movements of equilibration. This was observed throughout the course of the disease. The tendinous reflexes were rather lively especially in the lower extremities. There was no clonus and the plantar reflex was in flexion. No disturbance of the sensibility was found. The pupils reacted to light and accommodation, the eye movements were normal and nystagmus was not noted. The corneal reflex was diminished on both sides. A fundus examination showed choked discs in both eyes. An examination of the remaining cranial nerves presented nothing abnormal. A radiograph of the skull showed nothing abnormal, the outlines in the occiput being very clearly seen. In the differential leukocyte count there was an eosinophilia of 8%. An examination of the stool showed no parasites or ova. The urine was normal. The Wassermann reaction was negative in the blood and in the spinal fluid. The spinal fluid was under considerable pressure but an analysis showed a normal content. The Moro tuberculin reaction was negative. Galvanic vertigo was found to be present as in the normal subject.

The following tests, proposed by Babinski for disturbed cerebellar function were applied and found present except as noted:

Movements démesurés. If the walk be analyzed it will be seen that exaggerated movements of the lower extremities are a factor in the production of the unsteady gait. Individual movements are well oriented and the sight does not influence the movement. This was shown in the Romberg test.

Asynergia. If, in the sitting position, the foot be raised to touch the hand of the examiner held at some distance above the floor, the thigh will first be flexed to the required height and then only will the leg be extended to complete the desired movement. There is then a dissociation between the movements of extension and flexion.

Catallepsie cérébelleuse. In the reclining position the legs could be held quite steadily raised above the body—this in marked contrast to the great ataxia on attempting to walk. True catallepsie as described by Babinski and as shown in my other case was not present.

Adiadokokenesis. The difficulty in getting the child to perform the movements necessary for the carrying out of this test did not enable us to determine definitely as to the presence of adiadokokenesis.

Pointing tests of Barany. These tests were gone over by Dr. H. B. Graham in the ear clinic. They were found to be normal. The cochlea and vestibular apparatus were intact. Dr. Graham found a spontaneous rotary intermittent nystagmus present on this examination, which I was not able to find on subsequent examinations. From the results of these tests Dr. Graham expressed the opinion that the condition was one of cerebellar tumor not cortical in origin.

It was noted that the child had a tendency to hold the head inclined forward on the chest. A sudden passive motion of the head did not appear to be followed by an unpleasant sensation. It was not noticed that when the child fell that it was more often in any one direction.

We considered the diagnosis in this case to lie between a serous meningitis with pressure symptoms and a cerebellar growth. In order to exclude the possibility of the former condition it was decided to perform the operation of Anton of Halle—draining the ventricular cavities in the subdural space by puncturing the corpus callosum. This was accordingly done by Dr. Sol Hyman on June 5. After the first effects of the operation had passed there was marked improvement subjectively and objectively. The headaches disappeared, the child was brighter, the walk improved somewhat and the choked discs cleared up so that the margins of the discs could be clearly outlined. The trouble in locomotion was always marked, however, and was relatively little improved compared with the other symptoms.

The improvement noted was but temporary and on the 10th of August on account of a recurrence of the symptoms a diagnosis of cerebellar tumor was made and an operation to uncover the cerebellum was performed. This was to be done in two stages but the child died of shock following the first stage or the stage of decompression.

At the autopsy the cerebellum was found crowded down against the foramen magnum so that both tonsils of the cerebellum were pushed through it causing a deep groove on their surface. These markings are still plainly seen. Literally pressure grooves are also seen caused perhaps by the pressure of the sigmoid sinus. There was no projecting tumor mass on the surface of the cerebellum but it was noted that the region of the superior vermis was of a different color than the adjoining hemispheres, being of a much lighter color with an absence of definite markings. Section of the cerebellum showed a tumor mass—glioma—5-6 cm. in diameter with a cystic center which contained fluid under considerable pressure. The tumor mass involved the vermis and the white matter of the hemispheres adjoining including the dentate nuclei of both sides and extending into the central white matter of both superior peduncles. The superior worm was destroyed as was the inferior worm for the most part, only the inferior surface of the pyramid, uvula and the nodulus being intact. From the location of the growth and from its relation to the dentate nucleus the nucleii of the roof were unquestionably involved. Although the tumor mass approached the surface of the hemispheres to within 1 cm. of the surface posteriorly the striking part of the specimen is the conservation of the cerebellar cortex. This becomes of importance in referring back to the pointing tests of Barany which were not found to be abnormal; and the almost total destruction of the vermis is interesting in the light of the great disturbance of the function of equilibration and the relative conservation of the power of voluntary motor regulation as shown in the absence of any great ataxia in the finger nose tests.

The foramen of Megendie was patent and no basal meningitis or ependymitis was present.

Case 2. I will briefly present the second case history, laying special stress on the symptoms referable to the cerebellum. An intelligent man, aged 58, an inmate of the Relief Home, whose family history and past history present nothing of importance bearing on his condition, gave the following account of his illness:

In December, 1909, in Japan while sitting in front of a tea house in Yokohama he was suddenly seized with a sensation as if struck in the knees by a ball of lightning as he expressed it. He did not lose consciousness, was not dizzy and was able to walk a short distance unaided. He was put to bed and remained there a few days. Immediately following this stroke the trouble of which he now complains developed and has remained about the same in the three years which have elapsed. This trouble is chiefly the difficulty in controlling the movements of the left arm and leg, although the members on the right side of the body are also

affected to a lesser extent. Walking, or even standing, unsupported is impossible. He can feed himself but it is hard work. He has noticed that the character of the speech has changed, having become slow and deliberate, but there is no difficulty in articulation. The desire to urinate is imperative. He states that his memory has not suffered and that his mind is as clear as formerly. He has never had headaches or pain of any sort and he has not noticed that his strength has failed to any great extent. Vision for distance is good.

The status taken in December, 1911, showed a fairly well nourished man presenting a great incoordination of voluntary movements. To walk was impossible. Movements of the right upper extremity showed a moderate ataxia while coordinate movements of the left upper extremity were practically impossible. As it appeared to us that the type of ataxia was cerebellar in character special attention was paid to its study.

Movements démesurés. These movements were marked, in this case being evidenced by typical dysmetrie described by Thomas and Jumentié. When the patient attempted to place the finger on the tip of the nose the finger missed the mark by a wide margin, the movement being, nevertheless, well oriented and the error was of the same degree with or without the aid of the sight. In these two characteristics the ataxia was in marked contrast to the ataxia commonly seen in tabes.

Asynergia. This was present in the left lower extremity.

Catallepsie cérébelleuse. This was present in a very striking degree. In the reclining position the legs could be held elevated above the trunk quite motionless without a sign of the ataxia to be expected. Not only was there an absence of any oscillation but the members could retain this position for long periods without the usual fatigue constantly present in the normal subject.

Adiadokokenesis. This was marked in the left upper extremity. Successive pronation and supination of the left hand was impossible; in attempting this wide excursions of the forearm were made.

Pointing tests of Barany. There were no spontaneous errors in pointing. Pointing tests after turning by means of the rotary stool were not made. Among the signs of cerebellar disease was the scanning speech. This was a very prominent symptom in this case. There was, however, no difficulty in articulation. Voltaic vertigo was tested for. An interrupted current of from 14-16 m. a. was necessary to produce an inclination of the head, but it was always to the side of the positive pole.

It was not noticed that this patient had a tendency to fall particularly to one side when he lost his equilibrium nor was hypotonia present to any marked degree.

Reflexes: The radial, triceps, patellar and achilles reflexes were present on both sides and quite lively. The reflexes on the left side were found to be increased over those on the right. The plantar reflex was in flexion. The abdominal and cremasteric reflexes could not be elicited but the anal reflex was present.

The superficial sensibility was not affected and the sterognostic sense showed no impairment.

An examination of the cranial nerves showed very little. The pupils were equal in size and reacted to light, accommodation and convergence. The movements of the eyes were normal but it seemed to be an effort for the patient to look upwards and he complained of a pain in the back of the neck in attempting this movement. The fields of vision were apparently normal when tested roughly and there was no spontaneous nystagmus. The corneal reflex was perhaps a trifle diminished but distinctly present. The facial nerves were not involved. Hearing was diminished on both sides so that the ticking of a watch could only be heard in contact with the ear. There was no involvement of the 9, 10, 11 or 12 pair of cranial nerves.

On account of the history of sudden onset, the nature of the ataxia, and its prominence on the left side a diagnosis of a lesion of the left cerebellar hemisphere, vascular in origin was made.

The patient died suddenly on February 15, apparently from a stroke of apoplexy. At the autopsy when the brain was removed no tumor mass, thickening of membranes or adhesions were found, but it was noted that the left cerebellar hemisphere appeared smaller than the right and the corresponding posterior fossa of the skull was shallower than on the right side.

There was a marked arterio-sclerosis of the vessels at the base of the brain. The brain was hardened in formalin and afterwards placed in Muller's fluid. Sections showed two symmetrical areas of softening in the central white matter of both cerebellar hemispheres. The softening on the right side appeared recent. Another area of softening was found a little farther forward on the left side in external relation to the left superior peduncle. In hardening it will be seen that the inferior and superior peduncle on the left side have not taken on the brown color as on the opposite side. It would be premature to state positively before stained sections are made that these peduncles are degenerated but the gross specimen seems to indicate it. There was no evident lesion in any other portion of the brain.

In comparing the ataxia in these two cases it will be noted that the ataxia was far greater in this second case whereas the lesion was much less extensive. The question arises in this connection as to the result of slow or sudden destruction of cerebellar substance. In the case of sudden destruction as in softening it is possible that the coordinating function be permanently lost, while in the slower process the higher centers such as the cerebral hemispheres may assume this function by a process of reeducation.

Another question* is whether death by apoplexy may be caused by a lesion limited to the cerebellum. In the last case a complete autopsy was not performed so this point can not be settled in this particular case. In a case of chronic cerebro spinal meningitis observed recently, however, in which a complete autopsy was done the only lesion of sufficient importance to account for the sudden and unexpected death was a large area of softening in the roof of the 4 ventricle. This patient showed marked improvement in the meningeal symtoms prior to his death.

FACTORS IN THE PHYSIOLOGY OF BONE IN RELATION TO SURGERY.*

By ARTHUR L. FISHER, M. D., San Francisco.

My reason for presenting this paper at this time, is that it seemed to me from the discussions following Dr. Sherman's paper at the last

* Since the reading of this paper an article by Bernstein, "Kleinhirnblutung als Ursache plotzlichen Todes" (Deu. militararztl. Ztchr. No. 22, 1912. Abstracted in Deu. Med. Wochen. Dec. 12, 1912, p. 2379), dealing with this question has come to the notice of the author. A case is reported of death following a hemorrhage into one cerebellar hemisphere. Bernstein states that to his knowledge such a lesion has never been mentioned as a cause of death before, and he offers as an explanation secondary vascular disturbances in the nearby vessels of the medulla. This explanation would hardly be applicable to cases of softening. It seems to us more probable that in our cases where the lesion extended to both hemispheres the cause of death may be sought for in the loss of the co-ordinating function. In the case of cerebro-spinal meningitis mentioned above, bulbar paralysis appeared to be the direct cause of death.

* Read before the Surgical Section of the San Francisco County Medical Society, September 17th, 1912.

meeting of this section, that there was not a very definite understanding among those who are operating on bone as to what happens or may happen after the operation. To say that a nail or a screw will remain firm, or will get loose, or come out, without enumerating the surrounding conditions, does not indicate either a clear understanding of the subject or a disposition to credit the word of other men as to results observed by them. The foreign substances may remain firm or may get loose. It is a partial enumeration of the factors that influence this behavior that I shall attempt to give.

I have nothing new, nothing original, to offer though this is a subject that lends itself most readily to experimental study. The facts I here present I have picked up here and there, frequently in articles not dealing with the subject of bone physiology primarily, but incidentally mentioned.

The teaching and study of physiology is carried out along fairly well defined lines, but the physiology of bone is one that has not attracted any considerable amount of study. A search through the *Index Medicus* and the Surgeon General's Catalogue reveals a surprisingly small number of articles on this subject. In the *Index Medicus,* bone and muscle are grouped together but bone seems to be put in the heading largely as a matter of courtesy.

Bone is living tissue, a specialized connective tissue, but I fear that it is frequently regarded as so much hard substance, wood or what not, to be cut and sawed and have nails and screws driven into it; but as it is a living tissue, and not like so much dead wood it responds to stimuli, and responds in a definite way, and it is our business to study and try to understand the character of response to each set of stimuli, to analyze them and not take things for granted.

I take it that this is to-day a real live subject, for, as one author has recently said: "Bone surgery is coming into its own again." That is, it is coming to occupy the place formerly held by the ovaries and the appendix, and that is at present occupied by the tonsils, by which I mean that hundreds of people who get the opportunity, and simply because they have the opportunity, think they are justified in operating on bone.

Bone responds to stimuli and these stimuli are both chemical and mechanical. The chemical stimuli affecting the bones in general, as, in disorders of internal secretions, acromegaly, osteomalacia, etc., or in diseases such as rachitis I do not propose to deal with here, but only with chemical stimuli affecting bone locally which will be taken up later.

Among the physical effects comes: First and foremost the general biological law that constant pressure causes atrophy, and intermittent pressure causes hypertrophy. This is a well-known biological law, and needs no special demonstration to prove it here. Let us see how it may or can apply to bone. The experiment Dr. Sherman mentioned—the nail with the constant pull on it from a rubber band—illustrates a part of this law, namely, continued pressure causes atrophy. The constant pressure of the rubber band pressing the nail up against the bone caused an atrophy of the bone ahead of the nail. If the same condition were applied to the screw in a Lane plate the same would be true, the screw would loosen.

The other part of this law, intermittent pressure causing hypertrophy, can be seen illustrated by a patient with a slowly uniting fracture of the tibia, for example, in a plaster cast; kept flat in bed, the fracture unites slowly; allowed to get up and stump about on his cast and union will take place much more rapidly. It is illustrated again by the familiar fact that the strong muscles are attached into rougher and heavier surfaces and lines on bones than are weak muscles. This is in part (not wholly) due to the greater intermittent force applied.

Weight bearing lines in bone form heaviest where the force is greatest. This was emphasized by Julius Wolf.

In the correction of bowlegs, e. g., by pressure or osteotomy, the whole bone gradually straightens out. As weight is applied to the more concave part of the bone, the outer convex portion of the bone is absorbed, gradually disappearing entirely, and more and more bone is deposited in the weight bearing lines. It is this law also that is made use of in the correction of scoliosis.

That the local temperature has an effect is shown by the following extreme experiment reported by Rippert in the *Deut. Med. Woch.* 1909. He found that when the legs of animals were made bloodless, and then put in a freezing mixture for ten minutes, that the animals regained the use of the legs after a few days, but that the bone became necrotic; the cartilage, the periosteum, and the medulla remain living; the bone does not sequestrate but becomes covered with a new layer of thin bone, formed from the periosteum and the endosteum. This is, of course, an extreme procedure, but how frequently do we see bone made bloodless by an Esmarch bandage, exposed to lowered temperature for more than ten minutes. A careful series of experiments along this line would be a very valuable addition to our knowledge.

Also, Schepelman experimenting with hot air in the treatment of fracture in rabbits found that the hot air inhibited, to a certain extent, the formation of large callus.

The introduction of foreign substances into the tissues in general and also directly into bone, has, of course, been the subject of innumerable clinical experiments as well as some direct laboratory experiments. Lange of Munich, and his assistants Von Baeyer and Engelhardt, have studied foreign substances, particularly metals, and find that the chemical character makes a considerable difference, certain metals causing aseptic suppuration; for example, copper, and some metals being well borne; such as tin. They found also that electrical conditions must be taken into consideration, and if two metals are used aseptic pus is deposited around the negative electrode, for

example, when copper and zinc are used together the pus collects only about the zinc even though the copper alone always attracts the leukocytes. Thus it may be of considerable importance in the use of Lane plates, to consider the metals of which the plates and the screws are made.

Meisenbach of Buffalo, tried to demonstrate the influence of certain chemicals on the growth of bone. He injected various substances into the epiphysis of bone of young rabbits, and then studied them with X-Ray and histologically. The substances used were sterile water, sterile graphite pegs, staphylococcus vaccine, pure tincture of iodine, pure carbolic acid, pure alcohol, pure formalin, and two per cent formalin. With sterile water the results were negative, with sterile graphite pegs the results were on the whole negative, but one showed some thickening of the cortex and it is noticeable that the peg changed its position. The hole made by the canula always closed. In rabbits in which the graphite peg and staphylococcus vaccine were used together, six out of seven showed a thickening of the perichondral and endochondral bone in the diaphyseal region. No change was noticed in the epiphyseal line. Pure tincture of iodine showed no effect. The pure carbolic acid showed only slightly increased vascularity. Alcohol showed no change. With pure formalin all the rabbits showed a thickening of the cortex, irregularity of the epiphyseal growth, exuberant growth of the diaphysis, and a general widening and thickening of both epiphyseal and diaphysis. Two per cent. formalin showed similar changes, but not as extensive. His conclusions are as follows:

Bone can be stimulated to growth by chemical, mechanical and biochemical means. Mechanical stimulation chiefly affects perichondral bone formation, whereas chemical stimulation effects the epiphyseal line directly, causing proliferation of the cartilage cells and increases zones of provisional calcification and calcified matrix together with osteogenic tissue derived from the perichondrium Mechanical stimulation is slow, whereas chemical stimulation is rapid. The combination of chemical and mechanical stimulation increases both perichondral and endochondral bone formation. Retardation of growth may occur if the zone of provisional calcification is destroyed, or if this zone is invaded by excessive blood clot or by destructive process.

Of all the substances used formalin gave best results on account of its antiseptic properties, and its affinity for protoplasm. Formalin injected upon epiphyseal line, becomes an insoluble compound and therefore affects the epiphyseal line, both mechanically and chemically with the distinct local rather than a systemic tendency. It causes formation of osteogenic tissue by influencing the zone of provisional calcified matrix, and by increasing the number of osteoblasts from the perichondrium.

This work of Meisenbach, taken in conjunction with some done by Parsons and reported in the *Jour. of Anat. and Phys.* 1904, may throw some light on the formation of epiphyses. Parsons first points out that the epiphyses are not essential for (though they may be useful in) bone growth, as there are none in birds, except in one species. He divided all epiphyses into traction and pressure epiphyses—the traction epiphyses are those into which the strong muscles are inserted; and the pressure those in which the weight is borne from above. He also shows that it is the largest cartilagenous ends of bone that show epiphyses first, and that these occur in the center of the cartilagenous ball. At this point the pressure is greatest; now it is not only the pressure, as this would not explain why they occur in the largest first, but he believes that it may be that the centers degenerate first, and it is the blood vessels that grow in, in response to this degeneration, that carry the bone-forming cells with them. This may also be true in Meisenbach's experiments; that his chemicals cause slight necrosis, and in answer to this, the blood vessels grow in, carrying along the bone forming cells.

In the treatment of ununited fractures there is much more to consider than the mere approximation. Approximation does not mean union. Non-union occurs in 1 to 2 per cent. of all fractures, and it is only a small percentage of this 1 to 2 per cent. that does not unite on account of mechanical obstacles, interposed soft parts or a distance between the fragments.

Metal sutures of all sorts probably increase the normal softening that occurs about a fracture, and then the suture fails to hold. Also if the periosteum is ripped up it adds to the softening process, delaying union. If bones are cut they should not be sawed off too neatly, but rather leave slightly roughened edges to allow a certain amount of blood clot which will cause hyperemia, as in ununited fractures increased blood supply seems to help.

In all cases of operative procedure on bone, and particularly fractures that are operated upon, let us try to be a little more accurate in what we are doing and let us try to study the surrounding conditions so that we can arrive at some definite conclusion as to the value of given procedure.

Discussion.

Dr. Raymond Russ (presiding): Dr. Fisher is certainly to be congratulated upon this paper. If there is one subject that provokes discussion, it is the use of the Lane plates in fractures. Each man takes his own experience, and bases his conclusions on them. I have thought for some time that a study of the physiology of bone in relation to these plates would be most interesting. I think the Society should thank Dr. Fisher for his paper.

Dr. Samuel Hunkin: I am sure that I do not agree with Dr. Fisher in some of his statements, regardless of the authorities which he has cited. If I had a fracture, the nearer it could be gotten into apposition, and the more true it could be cut, the better I should be pleased, and less callous and more rapid union I should expect. I am not disagreeing with the experiments recited by Dr. Fisher, but with the conclusions he has drawn from them. I do not believe, for instance, a fracture of the tibia under ordinary circumstances would unite quicker if a fellow walked around on it than if it was kept quietly and securely in

a splint. When union is delayed, however, then we know that weight bearing stimulates the production of callous and favors union, but not a neat repair as we like best. Dr. Fisher also made the statement that bone is easily rendered bloodless by Esmarch bandage for a long time. Now it is the hardest thing in the world in my experience to make bone bloodless unless you control the artery above the nearest proximal joint, and it does not get so very bloodless even then.

I do not see, under the reasoning given, how Dr. Fisher accounts for the increase of growth which sometimes follows tuberculosis, especially in the lower end of the femur. Personally also I believe that the epiphysis is a mighty mportant thing in the promotion of growth, much more so that Dr. Fisher would have us believe. Apparently in children, it is essential to growth, and if anything interferes with it the growth comes mighty close to stopping. I do not know how it is in birds, but if it is as Dr. Fisher says, I would not care to have my child treated like a bird could be treated for injuries around an epiphysis.

Dr. A. L. Fisher, closing discussion: In reply to Dr. Hunkin, I am not decrying accurate approximation—what I said was that union does not depend upon approximation.

With regard to the bone being bloodless, I know it is difficult to make it absolutely bloodless; but a relative shutting off of the blood supply is not difficult to accomplish.

About tuberculosis: I think the answer to that (this is purely my own idea) is that the chemical products formed by the tubercle bacilli in and about the joints stimulate the bone to growth. Whether the stimulation of cells is increased by the chemical products of the tubercle bacilli, or by the necrosis of the bone and the new growth in response to the necrosis, is more than I attempt to say. The experiments of Meisenbach are suggestive in this way. He injects substances and the bone becomes thickened. The growth is irregular, but he gets a great deposit about the point of injection.

THE ARNETH BLOOD COUNT IN PULMONARY TUBERCULOSIS—A REPORT OF 80 CASES.

By R. S. CUMMINGS, M. D., Los Angeles.

In 1896 A. M. Holmes[1] noted there was a morphological change in the neutrophile polynuclear leukocytes in tuberculosis. Arneth,[2] however, in 1904 was the first to note a definite relation between the state of the nucleus and the patient's condition.

He divided the neutrophile polynuclears into five classes, according to the number of neclei each contained. Class I containing one nucleus, class II containing two nuclei, class III containing three nuclei, class IV containing four nuclei and class V containing five nuclei. He found in the normal person the following average polynuclear neutrophile pictures: 5% of cells fell in class I, 35% fell in class II, 41% fell in class III, 17% in class IV, and 2% in class V. In tuberculous patients, however, the count was frequently as follows: 12% in class I, 48% in class II, 30% in class III, 1% in class IV, while Class V contained none. This shows a marked increase in the cells with one and two nuclei with a corresponding decrease in those with four and five.

Arneth believed that tuberculosis could be diagnosed by an increase of the cells containing one and two nuclei, a condition which he termed shifting to the left.

In this country Klebs[3] in 1906 confirmed Arneth's findings regarding a shifting to the left in tuberculosis.

In 1908 Bushnell and Treuholtz[4] reported a series of observations both upon normal and tuberculous persons. They established what they termed an index by adding classes one and two and half of class three of each 100 cells, which facilitated a comparison of observations. They found their index for normal persons to be 67, i. e., 67% of the cells fell in classes I, II and one-half of class III, while in the tuberculous the index would rise to a height of 85-90, showing a marked shifting to the left.

In 1909 Minor and Ringer[5] reported their findings which served to confirm Arneth's contentions. They were very enthusiastic over the result of this method in assisting to more correctly prognosticate the condition of the patient, which enthusiasm was somewhat moderated by further work as recently reported by Ringer.[6]

Later Reed-Lewis[7] and Miller and Reed[8] confirmed the observations of Arneth upon cases observed for many months. From counts upon thirty apparently normal persons whose average index was found to be 48, they concluded that anything from 45-55 could be considered normal.

They also found a marked deviation or shifting to the left in tuberculous patients, the worse the condition of the patient the greater the deviation.

Cohen and Strichler,[9] reporting observations made upon tuberculous patients, found that as a patient improved there was a shifting to the left in place of a shifting to the right as reported by other observers, i. e., they found an increase of class IV and V and a diminution of class I and II.

It is impossible to understand why their results were so adverse to those of other writers; one criticism, however, is that no Arneth counts upon normal persons were reported, thus establishing their normal index.

Kagan[10] also concluded from his observations that the Arneth count was unreliable.

Williams'[11] results average about the same as other observers with the exception that he found great variation in his normal counts.

The results of Miller, Lupton and Brown[12] were similar to those of the majority of observers. They concluded that the Arneth count could not be used as a guide to dosage of tuberculin.

Briggs'[13] observations tend to confirm Arneth's contentions. His normal index was 58, he having made 30 counts upon 17 apparently healthy persons. He observed a shifting to the left in active tuberculosis.

A vast deal of discussion and work has been produced among German observers, a rather complete bibliography of which is given by Miller and Reed and Schilling-Torgau.[14]

In all of our examinations 200 cells were counted, 100 on each of two coverslip smears. Blood was taken from the ear and very thin smears were made, great care being exercised to avoid rupturing

the leukocytes. Coverslips were passed through the flame just before smears were made, thereby preventing the condensation of moisture when coming close to the ear which interferes with the spreading of the blood.

Wright's stain was found most satisfactory. All nuclei connected by a thread only were counted as two, while those connected by a distinct isthmus were counted as one. When one nuclei was superimposed upon another they were counted as two. Those in which any question existed were carefully studied under a high power lens.

Our observations were made at the Barlow Sanatorium and have extended over a period of ten months, in which time 80 patients have been studied upon whom 206 counts were made. Thirty-six counts were made upon 22 apparently normal individuals, both male and female, between the ages of 20 and 50 years, thereby establishing a normal index.

No. persons studied.	No. counts made.	Highest Index.	Lowest Index.	Average Index.
22	36	66	43	55

This index agrees with the majority of observers. In those upon whom several counts were made a marked constancy was noted.

Of the 80 patients 26 were classed in the third stage (Turban) and 54 in the second stage. Those in the second stage were as follows:

No. patients observed.	No. counts made.	Highest Index.	Lowest Index.	Average Index.
54	124	81.5	33	60

Those in the third stage showed as follows:

No. patients observed.	No. counts made.	Highest Index.	Lowest Index.	Average Index.
26	82	92	52.5	73

It will be noted that there is a wide difference between the highest and lowest indices.

This can be understood when we consider that patients having a very slight pulmonary involvement in addition to a slight middle ear or larynx tuberculous inflammation must be classed in the third stage. Also patients who have cavity formation and yet are in good condition and rapidly recovering are still classed in stage three.

In second stage patients with high counts, some were counted after an acute illness, as pneumonia, yet were still in stage two. Therefore the average count is the one which gives the most correct general information.

Of greater interest is the classification according to the prognosis. The 80 cases were divided into three classes, viz: good, questionable and bad, as taken from the records of the attending physicians. The following table shows:

Prognosis.	No. patients.	Highest Index.	Lowest Index.	Average Index.
Good	53	83	33	60
Questionable	10	78	50	67
Bad	17	92	52.5	77.5

Numerous cases might be cited showing a gradual decrease of the index as the patient improved or an increase as the patient became worse. In general, however, it was observed that there was very little change from month to month, unless some acute complications, as a serious hemorrhage or pneumonia, intervened, in which case a rapid increase of the index was observed. In no case did we observe a raising of the index as the patient improved or a lowering as the patient became worse, except in two far advanced patients a short time before death in which the index gradually became lower.

The following conclusions seem to be justified:

1. The Arneth count has no diagnostic value but assists in prognosis only.
2. In extensive tuberculous inflammation of the lungs there is an increase of the neutrophile leukocytes containing one, two and three nuclei at the expense of those containing four and five nuclei.
3. As a patient becomes worse the index gradually becomes higher.
4. As a patient improves his index gradually becomes lower.
5. An index over 75 as a rule is found in patients with a bad prognosis.
6. That if one follows the Arneth count absolutely he will frequently find himself mistaken, but it will be found of positive value in questionable cases as corroborative evidence only.

In conclusion, I want to thank Dr. W. Jarvis Barlow and the attending staff of the Barlow Sanatorium for their kindly interest and their many helpful suggestions.

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ON THE DIFFERENTIAL DIAGNOSIS OF APPENDICITIS AND NEPHROLITHIASIS.*

By M. KROTOSZYNER, M. D., San Francisco.

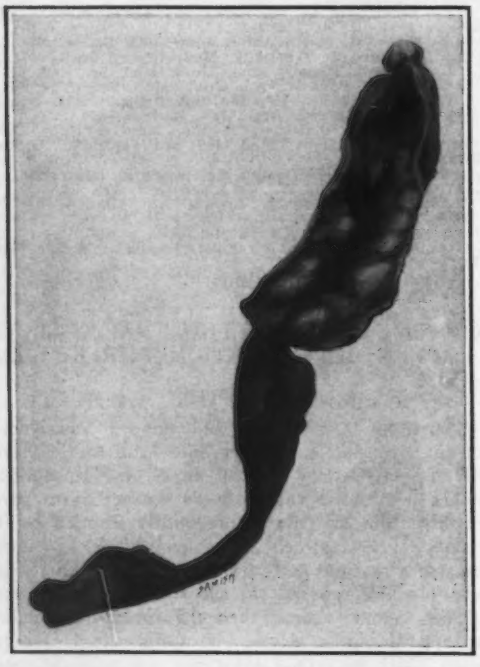
Attempts to recognize and remove concrements of the upper urinary tract date back to the dawn of medical history at the times of Hippocrates, when perforated calculi were extracted or paranephritic abscesses were successfully emptied by incision. The treatment of nephrolithiasis, of course, became a rational and scientific procedure only since Simon in 1871 paved the surgical road to exposure of the kidney. Since then the literature contains an appallingly large amount of individual observations which have shed much light upon the anatomy and clinical pathology of nephrolithiasis. Particularly, though, since the perfection of radiographic technic the recognition of lithiasis of the upper urinary tract was so simplified, that, in the great majority of instances, the correct diagnosis can be made, at present, by the average well-trained practitioner.

Equally well, if not better established is, at present, the diagnosis of appendicitis. The symptomatology and clinical pathology of this disease

* Read before the Surgical Section of the San Francisco County Medical Society, March 18, 1913.

appear to be so deeply rooted in the minds of the profession—especially in this country—that almost every symptom located at the right lower quadrant of the abdomen is apt to arouse the suspicion of being due to an affection of the appendix. Normal appendices are, therefore, removed in many instances, while other pathological conditions are not unfrequently overlooked. Instances, on the other hand, in which an unsuspected appendicitis at operation is found to be responsible for a symptom-complex pointing to an entirely different condition, are still comparatively rare. Therefore the detailed report of the following case appears to be justifiable:

A woman of 30 was, about 9 months ago, transferred from the gynecological to the urological service of the German Hospital. Her family and previous histories were unimportant. Menstrual flow started at the age of 14, which, at first, was normal in amount and duration but, during the last few years, became scant. She has had two miscarriages, both of them induced; the first occurred 10 years ago, when the patient carried the fetus up to eight months, and the second one year ago, when a three months' fetus was removed. She was curretted three years ago on account of a yellowish vaginal discharge which was accompanied by fever



and headache. Since her first miscarriage the patient suffered from intermittent attacks of pain in the small of the back, which were aggravated by hard manual work. She had to get up three or four times a night to pass water, but the act of urination, at that time, was not connected with pain or burning. She entered the hospital two or three months prior to coming under my observation, where, at the gynecological service, a right-sided salpingitis was diagnosed. A laparatomy was performed, the right adnexae were removed and the position of the uterus was remedied, the wound healing by first intention. While convalescing from this operation the patient had repeated chills and fever connected with pain on micturition which was agonizing in character. Therefore she was removed to the urological service.

Status: General examination negative. Kidneys not palpable. Patient complains of sharp pains on right side of back in lumbar region, radiating to the lower part of abdomen and down to the inner aspect of right thigh, as far as the knee. The pain is always present, being dull in character while the patient is at rest, it is accentuated by active or passive movements of the right lower extremity. Urine cloudy, containing abundant pus microscopically. Cystoscopy shows the picture of a subacute cystitis. Meatoscopy demonstrates normal spurt on the left side, while the right ureteral orifice apparently goes empty. The ureter catheter passes on the left side up to the renal pelvis without obstruction, while the right ureter can only be catheterized to a point about 15 cm. distant from the vesical meatus. The obstacle located at that point cannot be overcome by lower-calibred catheters or ureteral bougies. No urine is flowing from the right ureter, nor can fluid be obtained from the right side by the usual maneuvres. Chromocystoscopy with indigo-carmine demonstrates blue spurt on left side within 6 minutes, while no color appears on the right side within an hour. The usual functional tests show normal values on the left side and, microscopically, nothing of pathological note. Comparative functional tests were obviously not feasible because no urine could be obtained from the right side. Blood cryoscopy showed—0.549. Tests for determination of total renal function by means of phenolsulphonephthalein and phloridzin give normal values. Radiography shows apparently normal kidney-shadows on both sides, and two small well defined shadows on the right side of the spinal column at the site of the ureteral impediment and apparently in the course of the ureter as ascertained by a shadow-casting ureter catheter.

With this evidence at hand the obstruction of the lumen of the right ureter was considered to be due to a concrement, the operative removal of which by ureterotomy was decided upon.

Operation: General anesthesia. Incision parallel to Poupart's ligament and extending seven to eight inches upward towards the right lumbar region. Incision carried down to peritoneum, which is pushed towards mid-line, until the large iliac vessels and the right ureter are exposed. The latter is found to be imbedded in dense adhesions particularly at the point of its crossing with the ext. iliac. In the attempt to free the channel from these adhesions the peritoneal cavity is opened and the long and tortuous appendix is found to be a part of the adhesions upward and downward until a point near its insertion to the bladder. Suture of rent in peritoneum; wound closed by layers without drainage.

The appendix when opened, showed in its center several fecal concretions, which corresponded to the shadows on the plate. (See Fig. below.)

The patient rallied well from the operation. The convalescence was complicated the first week after the operation by a slight leakage from the right ureter; otherwise uninterrupted recovery.

About six weeks after the operation the right ureter could be catheterized to a point about five cm. from its renal pelvis, while the obstruction encountered before the operation could be easily passed. The right-sided renal secretion is still cloudy, containing, microscopically, pus, while the left-sided urine is clear and, microscopically, normal. Urea on right side 0.002; on left side 0.011. Phthalein appears on the right side after 22 and on the left side after seven minutes, its quantity during one hour is 38% on this side, while only a trace is recovered from the right-sided urine during the same period of time.

At later cystoscopic sittings, at monthly intervals, two, three and four months after the opera-

tion an obstruction of the right ureter is encountered at the same point as before the operation. No fluid can be obtained, no color can be recovered after Phthalein from that side. Since, though, the patient does not suffer any discomfort from micturition and since her general condition is excellent, she cannot be persuaded to submit to further examinations.

The literature contains but scant reference to like or similar observations as that recorded above. The most noteworthy publication dealing with the differential diagnosis of appendicitis and lithiasis of the right upper urinary tract seems to be that of Brown, Englebach & Carman [1] who have carefully studied the various relations of the appendix to the right ureter by anatomic and stereoscopic skiagraphs on bodies whose appendix was injected with mercury, while stylets were lying in the right ureter. The most important relation of these two organs lies, as proven by these radiograms, at the brim of the pelvis just anterior to the right sacro-iliac synchondrosis. With the cecum in the normal position this point corresponds to the base of the appendix and the location at which the ureter crosses the ileo-pectineal line. This point of the appendix is separated from the ureter by the peritoneum only. It is thus easy to conceive how a lesion of the appendix, which has extended to the peritoneum, can involve the ureter by contiguity and the assumption to recognize pus, albumen and blood in the urine as caused by contiguous inflammation of the right ureter in an individual suffering from appendicitis, appears to be justifiable.

My own case illustrates clearly that, under these circumstances, concretions in the appendix may have the same location, shape and size on the plate as ureteral calculi and vice versa.

It is probable that Kelly's wax-tipped catheter, introduced with faultless technic, or a ureter-catheter with a telephonic attachment would have aided in the diagnosis in a negative sense. I do not think, though, that the correct positive diagnosis was feasible prior to the operative autopsy in vivo.

Discussion.

Dr. S. O. Beasley: I saw an interesting case in which the appendix and the upper end of the ureter were adherent to one another. This was a case on which Dr. Emmet Rixford operated and as I do not see him present I will take the liberty of referring to the case. This patient had among other symptoms a large amount of pus in his urine and pain in the region of the right kidney. I am not familiar with the other symptoms and signs which were present in this case. The region of the pelvis of the right kidney was exposed by the usual lumbar incision and a moderate sized abscess was opened just behind the pelvis of the right kidney. A stone about ¾ inch in diameter, if I remember correctly, was found in the abscess cavity. This cavity communicated with the pelvis of the kidney by a perforation in the posterior wall of the latter. Through this opening the pus around the stone was discharged into the pelvis of the kidney and so escaped by the ureter into the bladder. Projecting from the surface of the stone was a sharp piece of metal resembling the end of a tack or possibly a heavy needle, and further examination of the local conditions satisfied Dr. Rixford that the stone was an enterolith which had formed in the end of an appendix which occupied this unusual position. A radiograph in this case would probably have shown the metal on the stone and so would have indicated its probable origin.

Dr. Alfred Newman: In a case I operated a month ago, the patient suffered with pain on the right side for some time. The X-ray showed two shadows in the region of the right ureter, which was impossible to catheterize. An operation was undertaken to explore the ureter. The patient was found at operation to have appendicitis and pericolitis, and the shadows were caused by two glands in the mesentery. The glands were slightly calcified. The patient recovered and the pain disappeared. This would show another case where shadows superimposed in the course of the ureter simulate urethral stone.

Dr. W. W. Boardman: The X-ray catheter may be of great aid in differentiating ureteral from extra-ureteral shadows. In the case of a supposed ureteral stone, the X-ray catheter would be passed into the ureter and two exposures made—one with the tube well to the right side, the other with the tube well to the left side. If, now, the shadow cast be due to a ureteral stone, it will of necessity lie in close proximity to the shadow cast by the catheter. If, however, the shadow be due to some formation lying in other planes than the ureter, there will be more or less separation of the suspected shadow and the catheter shadow in one or both of the radiograms. However, in a case such as the one reported, if it so happened that the stone lay very close to the ureter, recognition of its extra-ureteral origin might be impossible.

As regards the radiographic recognition of calcified glands and ureteral stones, will say that as a general rule, ureteral stones will be found to be more or less oblong or fusiform, of uniform density and with their long axis in the line of the ureter, whereas calcified glands are usually more or less circular in outline, of irregular density and if presenting a long axis, it rarely lies in the line of the ureter. Again, calcified glands are much more apt to be multiple than are ureteral stones.

As to the use of the waxed tip catheter in recognizing ureteral stones, the presence of definite scratching of the wax (the operator being sure of his technic) is certainly very strong evidence. However, Dr. Kelly would not rule out the presence of the ureteral stone on the negative evidence furnished by this proceeding, especially with the apparent positive radiographic evidence present in this case.

Dr. S. T. Pope: Two cases of a similar nature have come under my observation. The first was one in which all the physical signs and clinical history pointed to the presence of a ureteral calculus. The patient had repeated attacks of mild colic, referable to his right flank, extending down the groin, accompanied by vesical tenesmus. There was no fever, no leukocytosis, no pronounced local tenderness over the appendix. His urine contained microscopic blood and pus, with urate deposits. Cystoscopic examination showed pus from the right ureter. I suggested the possibility of a ureteral calculus, and proposed that an X-ray be taken. Within a few days, before he had time to attend to the radiography, he developed a typical attack of appendicitis. At operation it was found that his appendix was actively inflamed, and lay adherent to the right ureter, from which it was separated with difficulty. His recovery was prompt, and all renal findings disappeared.

The second case was one in which the onset of pain was confined to the right testicle—no fever, no frank signs of appendicular involvement, no leukocytosis. He had blood and pus in his urine. A diagnosis of ureteral calculus was made. On the second day this diagnosis was in question. The third day changed our opinion entirely, and, the Ochsner method of treatment then being in vogue, the case was conducted as one of ruptured ap-

1 J. Am. Med. Ass'n., May 7th, 1910.

pendicial abscess. The man died on the fifth day, and at autopsy a large enterolith, having a nucleus of hair, was found embedded in a fistulous communication between the appendix and the right ureter, at the brim of the pelvis.

THE SKIN REACTION AFTER COWPOX VACCINATION. A POSSIBLE AID IN PUBLIC HEALTH ADMINISTRATION.*

By JOHN NIVISON FORCE, M. D., Berkeley.
From the Department of Hygiene, University of California.

During the course of an anti-vaccination meeting held in Berkeley recently, the principal speaker of the evening issued a challenge for any person present to define successful vaccination.

The anti-vaccinationist, firm in the conviction that the medical profession still holds unchanged the hypothesis of Jenner, must of necessity find many weak points in the definition laid down in the present vaccination act.[1]

For obvious reasons no one in the audience had the temerity to take up the gauntlet thrown down by this knight of medical freedom, and his further statement that vaccination does not guarantee protection from smallpox found no dissenters.

This paper in an attempt to frame a definition of successful vaccination which, while probably not meeting the requirements of the challenger, will at least take cognizance of certain facts not usually recognized from a public health administrative standpoint.

In Jenner's original monograph (1798) the statement is made that the attempt to inoculate with variolous matter a person who had previously had cowpox, would result in a speedy efflorescence around the site of inoculation, which would fade away in a few days without further symptoms.

"It is remarkable," writes Jenner, "that variolous matter, when the system is disposed to reject it, should excite inflammation on the part to which it is applied more speedily than when it produces smallpox. Indeed, it becomes almost a criterion by which we can determine whether the infection will be received or not. It seems as if a change which endures through life had been produced in the action or disposition to action in the vessels of the skin; and it is remarkable too, that whether this change has been effected by the smallpox or the cowpox that the disposition to sudden cuticular inflammation is the same on the application of variolous matter."

Von Pirquet gave to Jenner's "sudden cuticular inflammation" the name "immediate reaction." He showed that inoculation of a previously sensitized individual with the organism producing vaccinia, would give rise to one of several related phenomena.

1. If specific antibodies were present in the blood of the individual, the vaccine matter would be promptly digested. Clinically this "immediate reaction" is manifested by a small areola of "efflorescence" with perhaps a papule which disappears by the end of 48 hours.

2. If specific antibodies are not present but the power of forming them still persists, the vaccinia organisms may begin to grow. This growth is checked as soon as the antibodies are sufficiently developed. Clinically we see a scale of appearances ranging from "early reaction" and "torpid early reaction" through many types of "accelerated reaction" or vaccinoid, depending on the length of time between the inoculation and the appearance of the antibodies.

The varying clinical pictures ranging from "immediate reaction" to true vaccinia must all, therefor, be regarded as successful vaccinations, since they are witnesses of the formation of antibodies and the consequent restoration of immunity. Figuratively speaking, the further the clock has run down the longer it takes to wind it up.

The following illustrative cases are taken from a series of eighty individuals examined with reference to the "sudden cuticular inflammation" of Jenner, or the skin reactions of Von Pirquet. These were chosen haphazard from over twelve hundred persons vaccinated during the months of January and February, 1913, at the University of California Infirmary.

1. W. P.—Had smallpox four years ago and is pitted on forehead, cheeks and nose. At the end of 24 hours had a very faint areola around the vaccinated point. At the end of 48 hours this had grown to a bright red areola measuring 15 mm. surmounted by a small papule. At the end of 72 hours there were signs of beginning papilla formation, and a final observation at the end of five days resulted in a diagnosis of vaccinoid.

2. J. B. P.—Had smallpox four years ago and is very deeply pitted, especially on the nose. At end of five days, when the normal vaccinia is just beginning to show papule formation, this case had developed a vaccinoid.

These two cases are examples of the "accelerated reaction."

3. H. F.—Vaccinated in 1905 and at present has a 10 mm. scar which has *no pits*. In 1907 had smallpox, and it well pitted on nose and forehead. Twenty-four hours after revaccination showed a 4 mm. areola around the point of insertion. At the end of 48 hours this had grown smaller, and at the end of five days had disappeared.

4. H. R. M.—Vaccinated between 16 and 20 years ago, and has a poorly marked scar, measuring less than 15 mm. Twenty-four hours after revaccination there was no areola around the point of vaccination. At the end of 48 hours areola of 5 mm. and a small papule. At end of five days a vaccinoid. Revaccinated some days later and showed both areola and papule at the end of 24 hours. At the end of 48 hours showed areola only, which had disappeared by the time of the fifth day observation.

These two cases are examples of the "immediate reaction" which in the case of H. R. M. followed an accelerated reaction.

* Read at the Forty-third Annual Meeting of the State Society, Oakland, April 1913.

1 Successful vaccination means that there has been evidence of a normal vaccinia, and that the person so vaccinated may be assured of immunity to smallpox for at least five years without repetition of the vaccination. Vaccination Act of March 7, 1911.

5. R. P.—Vaccinated over twenty years ago and has a small, well-pitted scar. During the course of some observations on the potency of vaccine virus, was revaccinated with three different strains of virus, the first two resulting in failures and the third in a vaccinoid. Before the vaccinoid reaction had reached its height she was revaccinated a fourth time in two spots with a control. At the end of 24 hours there was no areola. At the end of 48 hours there was no areola, and at the end of five days there had been no change in the spots, though the vaccinoid had meantime run its course.

This case shows that there is a time element in antibody formation. The third vaccination was destined to stimulate antibody formation, but the fourth followed so quickly that there were as yet no antibodies, and in consequence there was no skin reaction to be observed.

6. I. R.—Vaccinated within the last five years and has a small, well-pitted scar. Twenty-four hours after revaccination had a very faint areola. Forty-eight hours after showed 8 mm. areola. At end of four days 10 mm. areola, with slight papule, which faded without developing into a papilla. Revaccination a few days later gave a 7 mm. areola at the end of 24 hours, reduced to 2 mm. at the end of 48 hours, and subsequent rapid fading.

This case shows "torpid early reaction" which stands on the border between the early reactions and the vaccinoids. Revaccination gave the immediate reaction.

7. C. S.—Vaccinated within the last month, has three deeply pitted scratches 8 mm. wide by 40 mm. long. Revaccinated in two spots with a control. At the end of 24 hours showed 3 mm. areola around the control point, and 10 mm. areola around the vaccinated spots. At the end of 48 hours the areola had markedly decreased and thereafter rapidly faded.

This case illustrates "immediate reaction" in an extreme degree.

8. G. S.—Vaccinated several years ago, with no reaction at the site of inoculation, but claims to have had a generalized vaccinia, which left numerous shallow circular scars, slightly pitted, measuring 10 to 15 mm., distributed mainly on forearms and legs. No areola 24 hours after revaccination. At end of four days 8 mm. areola which proceeded to develop into the papilla of a normal vaccinia.

This case illustrates the value of a ready-made diagnosis.

9. V. N.—Vaccinated over 20 years ago and has a small keloidal scar. At the end of 24 hours had 5 mm. areola which developed into a papule at the end of 48 hours and had begun to subside at the end of 72 hours.

This case illustrates "early reaction" which is slower than "immediate reaction."

10. A. W.—Vaccinated between 16 and 20 years ago, and has a small, poorly-marked scar. Revaccinated in two spots with a control. At the end of 24 hours 3 mm. areola on the control point, and 6 mm. on each of the vaccinated points. At the end of 48 hours practically the same condition. At the end of five days a 20 mm. areola which developed into a vaccinoid. Again revaccinated after some days showed 8 mm. on the vaccinated points at the end of 24 hours, 6 mm. with slight papule at the end of 48 hours and only small brown spots at end of five days.

11. Vaccinated between 11 and 15 years ago and has a large scar. Revaccinated in three spots, observed at end of a week and recorded as a failure, there being only three tiny scabs to mark the spots of insertion. Again revaccinated in three spots at end of ten days from previous revaccination. In 24 hours 6 mm. areola around all 6 spots. At end of 48 hours still had 4 mm. areola which rapidly disappeared.

This case illustrates the effect of insufficient dosage, or "sleeping germs." The combined organisms of the two doses were able to call forth the immediate reaction which the single dose was not able to do alone.

Assuming that it might be of interest to read the observations on the series of eighty cases in terms of existing scars, two tables have been prepared to accompany this paper.

The first table shows the number of individuals in the series, giving each type of reaction, and further classes the reactions according to the observed attributes of the existing scars.

The second table is a similar grouping on a percentage basis.

TABLE I.

Immunity Reactions Classed According to Attributes of Scars.

Attributes of Scars.	Vaccinia	Vaccinoid or Accelerated Reaction.	Torpid Early Reaction	Early Reaction	Immediate Reaction	Sleeping Germs	Anti-Anaphylaxis.
NO SCAR	1	4		3	2		
CHARACTER:							
pitted		4	2	5	9		1
keloidal		5		5	9		
smooth	4	7	1	3	4		1
SIZE:							
small—under 15 mm.	2	7	1	11	15		1
medium—16-25 mm.	2	6	1	4	7		
large—26-40 mm.		3	1		2	1	
very large—over 40 mm.					1		
AGE:							
under 5 yrs.	1	1	1	2	3		
6-10 yrs.	1	10		3	5		
11-15 yrs.	3	3		4	11	1	
16-20 yrs.		2	1	2	1		1
over 20 yrs.		2	1	4	7		1

TABLE II.

Immunity Reactions Classed by Percentages and According to Attributes of Scars.

Scars.	Vaccinia.	Vaccinoid.	Immunity Reactions.
GOOD pitted & keloidal	0	23	77
POOR smooth	21	37	42
SMALL under 15 mm.	6	19	75
LARGE over 15 mm.	8	33	59
RECENT under 10 yrs.	7	41	52
AVERAGE 11 to 20 yrs.	11	19	70
OLD over 20 yrs.	0	14	86

In this latter table it is interesting to note that

individuals with small, well-pitted scars furnished the highest percentage of immunity reactions, as did the individuals whose scars were over twenty years old.

Let us now consider the administrative application of Jenner's observation as explained by Von Pirquet.

In accordance with the vaccination act, a California child must be revaccinated every seven years. If the revaccination fails the child is given a "due diligence" certificate good for one year. The average physician will not issue such certificate until he has observed the failure of two or more attempts at revaccination, usually six days apart. During the recent outbreak of smallpox in Berkeley, all unvaccinated children and all children subject to revaccination were excluded from the schools. This resulted in the loss of much school time and attendance money chargeable to the "due diligence" clause of the vaccination act.

A different story would have been told if these tests of immunity had been in recognized use. Three observations following the vaccination at the end of 24, 48 and 72 hours respectively, would have given information upon which a "due diligence" certificate might have been issued with a clear conscience. It would have been necessary to repeat the vaccination only in the few doubtful cases due to "sleeping germs." If the local school department rules prescribed the number of vaccinations necessary before issuing the "due diligence" certificate, the later ones could follow the first one in rapid succession if an "immediate reaction" has given clue to the immunity of the subject; while if the first one has given a vaccinoid the later ones will give the "immediate reaction."

A better plan, however, would be to class all these evidences of immunity as successful vaccinations, for indeed the immunity conferred must be the measure of the success. This would lead to the use of a definition of successful revaccination, based on the recognition of these principles. The appended definition attempts to embody these facts in some degree.

Successful vaccination is defined as visible evidence of a normal vaccinia; provided, however, that if the person under observation has had smallpox or has had a previous vaccination, visible evidence of a modified vaccinia (sometimes known as and called vaccinoid), or evidence of any recognized reaction of immunity against vaccine, shall constitute successful vaccination.

CHRONIC DISEASE OF THE GALL-BLADDER AND APPENDIX AS ETIOLOGIC FACTORS IN THE PRODUCTION OF DIGESTIVE SYMPTOMS.*

By W. FRANCIS B. WAKEFIELD, M. B., M. D. C. M., San Francisco.

Normal gastro-intestinal function is disturbed to a greater or less extent by any pathologic condition within the abdominal cavity. At times this functional disturbance is noticed merely as an accompaniment of manifest local disease somewhere; at other times the disturbed physiologic function of the gastro-intestinal tract is the predominant feature to such an extent as to obscure concurrent local pathology, and the associated, and probably etiologic, local lesion is apt to be entirely overlooked. This latter class is the one particularly pertinent to the present discussion. A maximum amount of general, chiefly gastro-intestinal, symptoms; a minimum amount of local signs.

The profession as a whole has been slow in recognizing the fact that most cases of chronic indigestion, particularly those characterized by periodic acute or subacute exacerbations, have as their causative basis a chronic inflammation of either the gall-bladder or appendix or both. Valuable articles dealing with this subject have appeared from time to time in our medical literature. They seem, however, to have failed to make the general impression their worth deserves. This is evidenced by the fact that so very many of these cases receive at the hands of their attendant physician a long course of desultory treatment for indigestion without any, or at best but transient, improvement of symptoms, while the real etiologic pathology remains unrecognized, often remains unsought for, until the patient, disgusted and discouraged, seeks new advice, and finally the real cause of the trouble is discovered and remedied.

As a matter of fact the vast majority of the patients who are now being treated for chronic digestive disturbances, who have been coming to the physician's office for several months, sometimes better, sometimes worse, are suffering either from a chronic ulcer of the stomach or duodenum, or from the gastro-intestinal phenomena which represent chronic intoxication resulting from the absorption of semi-toxic inflammatory products in some organ closely associated with the gastro-intestinal canal. Nine times out of ten the focal points are the gall-bladder and appendix.

The local findings are very variable. In most instances a marked tenderness on deep pressure is more or less constantly present. At times patients are conscious of right-sided pain or discomfort. In many cases, though, the local signs are negligible and correct conclusions can be reached only by painstaking investigation and intelligent exclusion.

To differentiate between a chronic ulcer of the duodenum and a chronic cholecystitis sometimes presents difficulties. Usually, however, the symptoms of duodenal ulcer are very clear-cut: the characteristic pain; its relationship to the ingestion of food; its complete relief on again taking food; its recurrence after two or three hours; the presence of occult blood in the stools.

Many writers lay a good deal of stress on the evidence produced by attacks of gall-stone colic and the presence of jaundice as means of discrimination between cholecystitis and duodenal ulcer. I wish, in the most emphatic terms possible, to decry the value of these purely accidental symptoms. Their presence certainly points to disturbance of the gall-bladder or ducts, but their absence does not imply that the gall bladder is free from disease. Jaun-

* Read before the Forty-third Annual Meeting of the Medical Society, State of California, Oakland, April, 1913.

dice, associated with cholecystitis, simply means that there is allied obstruction of the hepatic or common bile duct. The percentage of cases in which this occurs is comparatively small. Typical attacks of gall-stone colic point to the fact that there is associated cholelithiasis, and, furthermore, that the stones are sufficiently small to be forced, by contraction of the gall-bladder, into the cystic duct. The gall-bladder is quite frequently the seat of chronic inflammation; is unable to empty itself, owing to the viscosity of its fluid contents; yet no stones are present. Even when stones are present the majority of the cases are not characterized by typical attacks of colic because the stones are not small enough to be forced into the cystic duct. The gall-bladder may itself contract on a big stone and give rise to attacks of pain, but not the characteristic, excruciating pain that marks the passage of a stone through the ducts.

In working out these cases, the stomach contents, after a test meal, should be analyzed. Not much weight, however, can be placed on the results. The evidence thus procured is sometimes corroborative. Should we find complete absence of free hydrochloric acid we consider a possibility of gastric carcinoma and search for other evidence. We find this symptom, though, not infrequently, in chronic appendicitis. Again, hyperacidity, the common accompaniment of ulcer, is also found in most cases of chronic appendical or gall-bladder disease. The degree of hyperacidity might sometimes be of value as a means of differentiation in doubtful cases.

To those trained to read correctly the evidence presented, much valuable information can often be obtained from X-ray plates.

To differentiate between chronic cholecystitis and appendicitis is often difficult, sometimes impossible, always unnecessary. It is quite sufficient to decide that a given set of symptoms are probably due to a chronic inflammation of either the gall-bladder or appendix or both and to advise remedial surgical measures. If other abdominal or pelvic pathology has been carefully excluded, an incision along the outer border of the right rectus muscle extending from the costal arch to a point opposite the umbilicus will permit of careful examination of the cecum and appendix, of the duodenum and stomach and of the gall-bladder and ducts and through this incision both the gall-bladder and appendix may receive any requisite surgical treatment.

The general symptoms most commonly noted as the result of the systematic intoxication which accompanies chronic cholecystitis and appendicitis are recurrent attacks characterized by loss of appetite, indigestion, epigastric pain or discomfort and constipation. Accompanying these gastro-intestinal symptoms there is usually, in greater or less degree, general lack of musculo-nervous tone and mental depression. A certain amount of discomfort after eating, constipation and a general lack of muscular and mental vigor are apt to be present most of the time, but these patients are nearly always subject to distinct, more or less acute, exacerbations, at which times all the chronic symptoms are greatly exaggerated. Sometimes right-sided discomfort is complained of.

While the above clinical picture is that most frequently observed, we must remember that chronic pathologic conditions of the gall-bladder and appendix will frequently cause unusual reflex symptoms. For instance, occasionally we encounter recurrent hemorrhages from the stomach or from the urinary tract associated with chronic appendicitis.

In conclusion let me urge that we cease to consider our duty fulfilled when we have given a little dietetic advice and a dose of pepsin to patients complaining of symptoms of disturbed digestion. No class of patients is more deserving of careful diagnostic study and yet, in general, receive so little. A thorough study of these patients is our imperative duty and always must we consider the gall-bladder or appendix as possible etiologic factors. Further, when careful investigation fails to disclose any pathology in the stomach or intestines themselves, or elsewhere, to account for the symptoms, and when the symptoms continue in spite of thorough general treatment, we should then advise surgical exploration of the gall-bladder and appendix and appropriate surgical treatment of whatever pathologic condition is discovered.

STATE CARE FOR CRIPPLED CHILDREN IN CALIFORNIA.

By DOUGLAS C. McMURTRIE, New York.

In every community there are a large number of crippled children who because proper care is not provided for them are unable to take a useful part in activity or work of any kind. The crippled child who is not able to get about easily is denied the privileges of the public school and grows up in comparative ignorance unless the parents are able to make exceptional provision.

The deformities responsible for the crippled condition are often not acute and so do not receive the hospital treatment they deserve. Furthermore the term of treatment generally lasts over a long period such as the average institution is not able to give in view of the other acute demands made upon it.

Orthopedics is a specialized branch of surgery and adequate attention can generally be secured only in the larger centers of population. Thus patients living in the country or in small places are often likely to go unattended or at least to have their deformity so develop that it is no longer susceptible to effective treatment and cure. To gain the best results cases should be taken in hand early when the chances of recovery are infinitely greater.

Other classes of the handicapped are fairly well provided for in most communities. The blind, the deaf, the mentally defective—for all these there are institutions adequate, or nearly adequate, to the needs. But up to the present time the needs of the crippled child have not been properly provided for. From the economic standpoint only the provision of proper care is expedient because in many cases complete cures can be effected and in others the children can be furnished such primary and industrial educational facilities as will enable them to become useful and self-supporting members of the community. Without the provision referred to a great many would be helpless dependents for life.

It is interesting to note that several states of this country have made legislative provision for crippled children and established institutions where they could be given both surgical and educational

advantages. In this work the United States occupies a unique position. The first state to take such action was Minnesota, which in 1897 established a hospital and home for crippled children. The State of New York followed the example and established the New York State Hospital for the Care of Crippled and Deformed Children in 1900. Massachusetts started a similar institution, the Massachusetts Hospital School, in 1906, and several other states have taken some action in behalf of their cripples. The results of these institutions have been excellent. There have been found a large number of crippled children in each of the states named who were in need of the care provided and who have since profited by it.

It is important that this system of care should be extended to other localities. As yet the State of California has taken no such action and I venture to suggest to the physicians of that state the desirability of such a move. Such institution would offer the advantages an average hospital would be unable to provide and would obviate the neglect of education so often coincident with protracted treatment. The service which could be rendered by a state hospital school would be valuable and it would prove indispensable to the physicians interested in the welfare of this class of handicapped children.

SOCIETY REPORT

CALIFORNIA ACADEMY OF MEDICINE.

The regular monthly meeting of the Academy was held in the Library of the San Francisco County Medical Society on Monday evening, May 27th. The following scientific program was given:

1. Puerperal Infections. A Clinical Study of Twenty-one Cases. A. B. Spalding. Discussed by Harold Brunn and H. J. Kreutzmann.

2. The Use of Citrate Solutions in the Prevention of Peritoneal Adhesions. Saxton Pope. Discussed by W. I. Terry, L. Eloesser and J. J. Hogan.

3. Clinical Demonstrations (Illustrated by Lantern Slides). H. T. Morrow. Discussed by Saxton Pope, T. C. McCleave and H. E. Alderson.

Refreshments were served at the close of the meeting.

No meetings of the Academy will be held during June and July.

BOOK REVIEWS

State Board Questions and Answers. By R. Max Goepp, M. D., Professor of Clinical Medicine at the Philadelphia Polyclinic. Second Revised Edition. Octavo volume of 715 pages. Philadelphia and London: W. B. Saunders Company, 1911. Cloth, $4.00 net; half morocco, $5.50. net.

The main purpose of this volume is to provide a convenient compend for the use of those who wish to prepare themselves for State Board Examinations, and it will be found very helpful. The additions to this second edition include principally questions of serum and vaccine therapy; the recent work in the serum diagnosis and treatment of syphilis; the new heart physiology; the myogenic theory and graphic methods of studying the phenomena of the circulation.

How to Collect a Doctor Bill. By Frank P. Davis, M. D. 98 pages. Cloth bound. Price $1.00. Physicians Drug News, Publishers, Newark, N. J.

This little volume contains a mine of humor. A few excerpts will suffice. Here is an unfortunate colleague who can't collect $10.00 for his services in seeing a babe to the other side of the Styx. He writes to the un-remitting parent: "Dear Sir: I am very sorry that you did not see fit to reply to my letters of July 15 and August 15. . . . I have often wondered how I would feel if I knew my little child was up in heaven, looking down at me with her angelic eyes, wondering why I did not pay the doctor who worked so hard all night to give her ease and to keep her with me. . . ." Further on we see that in trying to collect a bill "The personal matter must be fitting to the case. If your patients do not die, you might speak of 'the innocent little babe who will grow up into womanhood unpaid for.'" There's some fun in being a patient and not paying your bills if these are the kind of letters you are to receive. The book will help to pass a merry quarter of an hour. L. E.

Dawn of the Fourth Era in Surgery, and other short articles previously published. By Robert T. Morris, A. M., M. D., Professor of Surgery in the New York Post-Graduate Medical School and Hospital; Member of the American Medical Association, the New York Academy of Medicine and other national and local societies. Philadelphia and London: The W. B. Saunders Company, 1913.

This volume consists of a collection of articles which have already appeared in the medical press. The author decided to collaborate the articles in the form of a small book, as the requests received by him for reprints became so numerous. The volume consists of, in all, 143 pages, divided into twelve or more chapters. In his work Dr. R. T. Morris deals with such subjects as "The Hand of Iron in the Glove of Rubber," "The Advantages of Expeditious Surgical Work," "My Change of View in Appendicitis Work," "The Dawn of the Fourth or Physiologic Era in Surgery," and "The Choice of Procedure in Cases of Loose Kidney."

Pathological Technic. Included Directions for the Performance of Autopsies and for Clinical Diagnosis by Laboratory Methods. By F. B. Mallory, M. D., Associate Professor of Pathology, Harvard Medical School, and J. H. Wright, M. D., Director of the Pathological Laboratory, Massachusetts General Hospital. Fifth Revised Edition. Octavo of 507 pages. Illustrated. Philadelphia and London: W. B. Saunders Company, 1913. Cloth, $3.00.

"Infection and Immunity." By Charles S. Simon, B. A., M. D. Published by Lea & Febiger, Philadelphia and New York, 1912.

The avalanche of new names and theories in the field of Immunology and Bio-chemistry—the readjustment of old theories to conformity with recent investigations—has more or less left the practitioner in a state of bewilderment. New phases and applications of "Infection" and "Immunity"—the broadening field and newer nomenclature—"Chemotaxis," "Opsonins," "Allergia," "Antigens," "Cytolysins," "Anaphylaxis," etc., convey to the physicians but vague, misunderstood phenomena. The general use of these reactions in diagnosis and in treatment demand at any rate a fundamental conception of principle in the former instance, and the ability to wield with finesse a two-edged sword in the latter. Simon's work is distinctly elementary and in this is decidedly deserving of merit. The practicing physician would do well to re-stock his knowledge of immunity by leisurely absorbing its contents. My only regret is that the writer did not present his work in a less didactic manner. The presentation of the evolution of Immunology, both in a chrono-

logical and suggestive sense, would have been a well rewarded placebo. E. A. V.

Diseases of the Heart and Aorta. By A. D. Hirschfelder, M. D., Associate in Medicine Johns Hopkins University. J. B. Lippincott Co., Philadelphia. Price $6.00.

It is particularly gratifying to receive another edition of this important book. The first edition became so well known and was found so useful by practitioners, students, and laboratory workers that it is hardly necessary to describe the second edition more than to say that it shows evidences of having been thoroughly gone over and amplified in accordance with the latest additions to this field of knowledge. For example, there is a concise but complete and extremely lucid description of electrocardiography, including the underlying physics, the physiology and the applications; the modern more exact indications for digitalis medication; the use of oxygen, carbon dioxide, rarefied air, etc. Few books on special subjects enter fields where there is more new but scattered knowledge which needs to be brought together, and still fewer books do this so clearly and concisely and with so much authority. Bibliographies are given. Illustrations are plentiful and mostly original. Throughout the volume shows evidences of an enormous amount of painstaking work, and it forms one of the most valuable volumes on a reference shelf.

E. S. K.

Solidified Carbon-Dioxide. By Ralph Bernstein, M. D. Published by Betz, Hammond, Ind., 1912.

In this little book Dr. Bernstein has presented the subject of the utilization of carbon-dioxide snow in dermatological conditions in an interesting and comprehensive form. The author is to be congratulated on the admirable results he has obtained by the use of the remedy. The résumé of the factors concerned in the utilization of the carbon-dioxide are comprehensively explained and the description of the method of preparation is concise and complete. Dr. Bernstein has obtained remarkable results in the treatment of epitheliomata but I believe a more safe and rapid measure could be found in the use of the high-frequency spark.

The end results in the treatment of angiomata are satisfactory, but in most instances the rapidity of the cure could be enhanced and the inconvenience to the patient minimized by prolonging each application of the snow.

The temporary freezing produced by carbon-dioxide has little, if any, bactericidal action and, therefore, would theoetically be of little value in the treatment of carbuncles, which Dr. Bernstein advocates. The freezing lowers the resistence of the tissues and in the presence of bacteria offers an admirable field for infection. In the treatment of this class of cases it would seem that the Bier hyperemic treatment would yield better results. The satisfactory results produced by the use of carbon-dioxide in the treatment of keloid, leucoplakia, lupus erythematosus, nevus lipomatodes, lupus vulgaris and xanthoma tuberosum would warrant us in considering this the elective procedure in these conditions. In the treatment of verruca vulgaris more rapid and equally satisfactory results can be obtained by the use of high-frequency or electrolysis.

Dorland's American Illustrated Medical Dictionary. A new and complete dictionary of terms used in Medicine, Surgery, Dentistry, Pharmacy, Chemistry, Veterinary Medicine, Nursing, Biology, and kindred branches; with new and elaborate tables. Sixth Revised Edition. Edited by W. A. Newman Dorland, M. D. Large octavo of 936 pages, with 323 illustrations, 119 in colors. Containing over 7,000 more terms than the previous edition. Philadelphia and London: W. B. Saunders Company, 1911. Flexible leather, $4.50 net; thumb indexed, $5.00 net.

Dorland's American Illustrated Medical Dictionary needs no introduction to the American profession. It is neat, well illustrated, concise in its definitions, and contains all the terms one is likely to meet in a life-time. The fact that over seven thousand new terms have been added is assurance that it is keeping abreast of the times.

Handbook of Diseases of the Rectum. By Louis J. Hirschman, M. D. Second edition, 338 pages. Royal octavo, 172 illustrations, including four colored plates. Price $4.00.

This is the second edition of Hirschman's work, the first having appeared four years ago. It is a work written for the general practitioner who so often is unfamiliar with the special methods of examination and treatment of rectal conditions, and who frequently treats patients of this type without much satisfaction to them or to himself. Symptoms, methods of examination and office treatment are especially considered. The chapter on constipation with its excellent radiographic illustrations is clearly written and should be helpful to the average practitioner. The author advocates the use of albolene, and curiously enough, although there is added a chapter on stool analysis based on the work of Schmidt and Strassburger, no mention is made of regulin (agar agar and cascara) introduced by Ad. Schmidt, nor of oil enemas introduced by Kussmaul and Fleiner and so frequently used by the gastro-entrologist.

R. B.

Sexual Impotence. By Victor G. Vecki, M. D., Consulting Genito-Urinary Surgeon to the Mount Zion Hospital, San Francisco. Fourth edition, enlarged. 12mo of 394 pages. Philadelphia and London: W. B. Saunders Company, 1912. Cloth, $2.25 net.

No subject in the whole realm of medical science requires for its authoritative discussion such a wide personal experience as sexual impotence. The correct interpretation of an underlying organic or functional lesion of the sexual sphere is only possible on the basis of a thorough knowledge of the border-line conditions (neurological, urological, etc.). Considering, furthermore, the many divergent opinions regarding the efficacy of the various therapeutic measures advocated for the relief of sexual impotence, a somewhat rational treatment seems to be feasible only at the hands of an experienced and well-trained observer, like the author of this admirable book. Refreshing is the open, candid and fearless manner in which he deals with his delicate subject, the mere mention of which is more or less abhorred in a country still saturated with insular prudishness and bigotry. Characteristic in this respect is the introductory sentence: "No one denies that the sexual function is of very great consequence to the individual as well as to society in general, although one does not care to make this a subject of conversation."

Vecki's book does not represent a compilation of the work and views of others, it gives, on the contrary, an independent view of the question of sexual impotence from his own standpoint, which, in many instances, is entirely original. The great value of the book, in other words, lies in the fact, that it contains the author's personal observations and views on this intricate subject, which he offers in his own inimitable and tem-

peramental way. This factor, while not detracting from the scientific value of the book, differentiates it favorably from the many dry treatises of similar nature.

After a brief review of the anatomy of the sexual organs, the second chapter is devoted to the physiology of the sexual act, containing many valuable new facts and the author's personal observations upon the microscopic aspect of the sperm and its main constituent, the spermatozoa. More than half the book's space is devoted to the fourth chapter dealing with the various forms of impotence, while in the remaining chapters its diagnosis, prophylaxis and treatment are discussed. This rather dogmatical division of the material does not permit of a good survey of the whole subject nor of quick information upon a point in question and would certainly be benefited by some change or modification in future editions.

While speaking of changes, the hope is expressed that a number of orthographic errors will be revised in the future, which are particularly glaring in names of distinguished foreign authorities (Albaran, Lohenstein, Ultzman, etc., instead of Albarran, Lohnstein, Ultzmann). Almost unpardonable in a book written by a graduate of a German-speaking university is the misspelling of the discoverer of water-Endoscopy, who instead of the good German Goldschmidt is persistently dubbed "Goldsmith."

Against these few minor discrepancies, most probably due to a mere oversight, figure the many and notable merits of the book. The author's views on important questions like sexual excesses, onanism, deficiency or correct guidance as regard sexual life, etc., are sane and moderate. His knowledge upon aphrodisiacs testifies of large experience and sound criticism. Statements like "the further we advance in the study of the deep urethra and the pathology of the various sexual appendages, the less frequently we see cases of genuine sexual neurasthenia," and "there are so many phenomena of sexual neurasthenia that one life is too short for the study and observation of them all," prove his good judgment and unusually wide experience and will be corroborated by every co-worker in this delicate and difficult work.

In harmony with the author's genial temperament a certain buoyancy and optimism pervades the book that communicates itself to the reader; it is interesting, attractive and instructive from cover to cover; it should be found in the library of every practitioner who wishes to be informed upon the pathology and rational treatment of sexual impotence. M. K.

NO MEDICAL SCHOOLS.

The Homeopathic Medical Society meeting at Los Angeles made complaint that the state legislature had been dominated by a rival school of medicine. The reference of course was to the medical practice act. This is the complaint always made against any medical practice act which treats medicine as a science, rather than as a religion, but it seems somewhat curious to hear it from homeopaths. The medical practice act of California does not require any homeopath to take examinations in homeopathy, or any "allopath" (if there is any such thing), to take examinations in "allopathy," or in fact, any medical practitioner to take an examination in any subject on which there is any difference between schools. The regular physician may give one grain or one-tenth of a grain of calomel and the homeopathic physician may give one hundred thousand trillionth of a grain of the same substance, but the medical examination of California does not test either of them on either of these methods. What it does require is that both of them shall know enough chemistry to know what calomel is made of; shall know enough anatomy to know where it goes when it is swallowed; shall know enough physiology to know the functions of the organs through which it goes; and shall know enough pathology to know what is the difference between the functions of those organs when they are diseased and when they are in health. If they are also required to know something of bacteriology, that certainly is not inconsistent with the Hahnemann doctrine that all diseases are forms of the itch, for if that doctrine has any meaning at all it can only be interpreted in terms of modern bacteriology.

The whole purpose of modern medical requirements is simply to demand that those who wish to practice the healing art shall be sufficiently trained in the fundamental sciences. Homeopaths are so trained, and the graduates of their schools are able to pass an examination on these sciences. There is nothing in the medical practice act to interfere with an educated man of the homeopathic or any other school. The incidental fact that when physicians are educated they practice all very much the same sort of medicine, and that the schools disappear, is not the fault of the law, and is not regarded by any educated man as a fault.—Fresno Republican.

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You must bear in mind, gentlemen, that it takes advertising to get business and more advertising to hold it. The larger you become, the harder you must fight to hold your lead. Overconfidence is one of the greatest enemies of both individuals and corporations. Shortly before Napoleon went down before Wellington he thought he was invincible.—Printers' Ink.

NOTICE.

A recently published medical book, found on street car, has been left in the office of the State Journal. Owner may have same by proving property and paying for this ad.

NEW MEMBERS.

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Burke, W. P., Redlands, Cal.
McConnico, San Bernardino.
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Ryan, L. R., San Diego.
Marsh, O. G., San Diego.
Weinberger, Joseph, San Diego.
Crawford, W. W., San Diego.
Coburn, E. S., National City, Cal.
Ringolsky, Sol., San Francisco.

DEATHS.

Koenig, Theodore T., Portola, Cal. (Died in San Francisco).
Wickman, W. J., San Rafael.
Gosewisch, W. R., Los Angeles.
Ladd, Ira Bourland (Died in San Francisco).